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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XIV.

Phenomena of the capillary circulation.—Experiments on frogs.—Coagulation of the blood.—Effects of defibrination.—Delusive theories respecting inflammation.—Action of subcarbonate of soda.—Non-contagious nature of the plague.—Absurdities of quarantine.—Action of acids and alkalies on the blood investigated.

GENTLEMEN:—I promised in my last lecture to give some further account of M. Poiseuille's researches, and proceed to fulfil my engagement by quoting that gentleman's description of another of his experiments. It confirms the statements I have made respecting the exclusively passive participation of the capillary vessels themselves in the circulation through them. You will, from it, obtain new proof that no movement takes place in the capillaries without a corresponding one occurring in the arteries; and that every movement in the latter vessels, directed from the trunks to the minor branches, is transmitted through the capillaries without undergoing the smallest acceleration by any action of those infinitely minute tubes.

"A white mouse, about a month old, is pinned down to a thin plate of cork; the abdomen is then freely opened by an incision on the median line, and the small intestine and mesentery laid out for examination on a plate of glass. The velocity of the globules is seen to be greater in the arteries than in the veins; in the capillaries, where the form of those bodies is perfectly distinguishable, it is generally less than in the two other kinds of vessels: the movement is, at first, continuous, without either jerk or intermittence.—Forty minutes have now elapsed, the greater part of the intestine lies outside the abdomen; the animal has evinced much less irritability for the last quarter of an hour. The blood now moves with diminished rapidity in the arteries, capillaries, and veins, in the intervals between the contractions of the heart. The movement of the fluid has now changed from *continuous* to *jerking-continuous*.—Fifteen minutes later: the jerking motion is now much more marked; and the progression of the fluid is extremely slow during the intervals between the contractions of the heart.—Ten minutes later: the globules now stand still after each systole of the heart, and this intermittent movement exists in the arteries, capillaries, and veins. This repose is followed by a retrograde motion of the globules after each contraction of the heart; an oscillatory movement then takes place in all the arteries; no further movement is distinguishable in a great number of capillary vessels.—The animal has now been under experiment for an hour and twenty minutes, and has ceased to give any signs of life: the span of the oscillations increases considerably, and the globules retreat quite as much as they advance during these oscillations; there is a sort of struggle going forward between the weak contractions of the heart and the resistance made by the arteries to dilatation. At length the movement of the globules in the arteries is scarcely discernible; shortly after this the only movement in those vessels is of a retrograde kind; all is at rest in the capillaries, and a slow though natural movement persists in the veins. The passage of the blood from the branches of the arteries and veins towards the trunk becomes slower and slower, and a state of complete rest is established at the end of twenty minutes. It is plain that the arteries and veins contain a much less quantity of blood than at the outset of the experiment; as for the capillaries, their state in that respect seems to have undergone no alteration.

"*Remarks.*—The systolic movements of the ventricles continue after death in batrachians; this persistence of the contractions of the heart after death is also observable in certain mammalia, such as the dog, the rat,

the mouse, &c. These movements, which are tolerably powerful immediately after the removal of the organ from the body of the living animal, grow gradually less and less energetic. The animal, on which I have made the above experiment, was becoming weaker and weaker every moment during its continuance, from the effects of the operation it had undergone, and at the same time the heart's contractions gradually lost their power; hence the *jerking-continuous*, *intermittent*, and *oscillatory* movements that successively replaced the *continuous* motion. After the death of the animal the heart continued to beat; while it did so, the oscillatory motion persisted also, but in proportion as the contractions of the heart lost their force still more completely, the retrograde movement gained the ascendant in the arteries, in consequence of the retraction of the vessels which ensues when they cease to be dilated by blood arriving from the heart; and through this retraction of the walls of the vessels towards their axis, the retrograde motion was kept up in the globules for twenty minutes after the animal had expired."

Globules in a state of quiescence in a limb separated from the rest of the body of an animal, have been sometimes seen to become suddenly agitated by movements in various directions, and this when no suspicion could arise of the object-glass having been altered from its horizontal position, or of its having been shaken in the least. What is the source of these displacements—for you well know that I do not share the belief of those who look on them as spontaneous or voluntary? Nothing more or less than the manner in which the light falls on the microscope. In instances where such an appearance has been observed, there can be no doubt that the sun was at one moment overcast with clouds, which dispersed at the next; by the change in the state of the heavens the luminous calorific rays were enabled to reach the mirror, and, being reflected from its surface, to play on the web submitted to experiment. As liquids dilate more than solids, the blood finding itself, as it were, pent up in its vessels, in consequence of the change of temperature, rushes to whatever points offer the least resistance. To this want of adaptation of the dilatation of the contained blood to that of the containing walls, are to be ascribed the movements witnessed. The same phenomenon occurs when an artificial light, such as that of a wax taper, is employed; when the lighted wick is brought near the limb the globular movements recommence; they cease when it is removed. Nay, more, an effect of this kind is often produced by simply conveying the part under examination from a cool to a warm room. If the globules were motionless in the first, they would, in the other room, become the seat of a trifling motion, because the temperature or the medium in which they are now placed is higher than that of the room in which they were before examined.

But there is another prevalent notion respecting the movements of the globules, which, though extremely absurd, is still worth refuting. Suppose you have placed a drop of blood, previously dissolved in alkaline or sugared water, on the object-glass; you examine it, and observe some of the globules rising, others descending, and a third set oscillating hither and thither. But are these voluntary movements? No; they are a simple result of the action of the laws of equilibrium: the heavier go to the bottom; the lighter rise to the surface. The same phenomenon, precisely, occurs with particles of colouring matter suspended in distilled water; before the liquid becomes perfectly quiescent irregular movements take place, dependent, as in the case of the blood, on the specific gravity of the corpuscula suspended in it. Besides, the light often falls unequally on the different parts of the drop submitted to inspection; the most strongly heated points dilate the most, and hence oscillations follow. But again,—it is the usual habit, when we wish to separate the globules perfectly from the fluid in which they float, to spread out the minute stratum under examination on one plate of glass, and to cover it with another. In this case, too, movements are observed in the fluid, but these also depend on the establishment of the equilibrium. The globules rush towards the points where there is least pressure; the moment the pressure is equalised over the entire surface all motion of the globules ceases.

Hence, Gentlemen, wherever and however we study the progression of the globules, whether it be in thin tubes and in active motion in them, or having artificial obstacles to contend with; whether it be when they are removed from their vessels and spread out on a transparent surface, still the result is the same;—never, in any case, do we meet with anything indicative of their possessing a power of vital nature independent of physical laws. I am well aware that the contrary opinion has been, and still is, professed by many conscientious men: they have made experiments,—these I admit; they have drawn inferences from them,—these I refuse to accept. And why? —Because such deductions as theirs do not legitimately flow from the phenomena observed. It is a matter of perfect indifference to me whether the globules do or do not enjoy a special vitality; indeed, I should, of the two, rather wish, were it only for the prettiness of description, that all we are told respecting them were correct. The mind feels a kind of satisfaction in fantastic imaginings of the sort, and man loves to dwell on everything which can dignify him

in his own eyes; proud of his intellect, he would delight to detect in each particle of his body an intelligent existence. But we must accustom ourselves to the study, not of what we should wish Nature to be, but of what she is. We shall be the gainers by doing so, for the discovery of the true is always ennobling: we shall be the gainers thereby, for her works are infinitely more perfect than aught human imagination is capable of creating.

You recollect, Gentlemen, I presume, the result of my experiments respecting the influence of low and high temperatures on coagulation, which I made known to you in my last lecture. Struck, as I was, with the bold manner in which observers asserted that when blood is submitted to the action of an extremely low temperature it freezes without coagulating; and, further, that if, while in this condition, its temperature be raised, it reassumes the liquid form, and becomes organised into a normal clot, I felt anxious to repeat their experiments. With this view, after having plunged the glass tube I hold in my hand into a frigorific mixture marking —14° by Réaumur's thermometer, I let some blood fall into the vessel containing it; the blood presently became solid. After a certain lapse of time I withdrew the tube, and gradually raised the temperature of the mixture; but, although I watched the progress of the experiment with the most scrupulous attention, though I never once turned my eyes from the vessel, I did not see the blood reacquire its fluidity. Even at this moment it is, with the exception of a trifling quantity of serum which has escaped from it, in the same state as when I removed it from the frigorific mixture: I cannot even detect any appearance of increase or decrease in its volume. The discrepancy between these results and those announced by authors, affords me no little surprise; but, as such difference does exist, I cannot make up my mind fully on the subject for the present, and reserve my decision until we shall have the evidence of further experiment.

I am in the habit of causing the blood drawn from my hospital patients to be received into two vessels, of exactly the same form and size,—one empty, the other containing an aqueous solution of sugar, which possesses, as I have already made known to you, the property of precipitating the globules without dissolving them, and so allows us to follow accurately the different processes in the organisation of the fibrine. These two glass vessels contain some blood taken from a healthy woman in the eighth month of pregnancy; in the one you observe a red and consistent clot, and, to all appearance, a normal share of serum; in the other, in which I had put sugar and water, we have a distinct fibrinous frame-work, formed of long delicate filaments suspended in the liquid, and adhering to the parietes of the vessel. In this case there is no indication of an abnormal state of the blood. Here are two other vessels containing some blood taken from a female who has miscarried in the sixth month of utero-gestation. The patient is so extremely weak and anemic that her ultimate recovery is a matter of doubt; accordingly, you will remark a material difference in the contents of the four vessels: the two last present a soft, flabby coagulum, floating in an excess of serosity, while its fibrine is inelastic and fragile. However, Gentlemen, this is a mere comparison, which may or may not be well founded; on the one hand, we have a healthy woman, with seemingly normal blood; on the other, an ailing person whose blood is evidently altered in its properties. This is a new point of view in which the study of the blood is likely to prove fruitful in pathological inferences, and we will avail ourselves to the full of it. I may, even now, point out to you the resemblance existing between the abnormal blood which I have just shown you, and that of animals partially defibrinised.

This brings me directly to the subject with which I have of late occupied your attention, namely, the effects of defibrination of the blood. You remember that the coagulable element of that fluid is, when gradually removed, reproduced with marvellous rapidity; the nature of this phenomenon has hitherto escaped our diligent researches, but we have at least ascertained that the fibrine found in animals from whom part of that contained in the blood had already been removed, is singularly modified; it is puffy, spongy in appearance, lighter, and yet more voluminous than normal fibrine; like albumen, too, it liquefies at a temperature of 60° centigr. Feeling most curious on this subject, and therefore desirous to push inquiry relative to it as far as possible, I bled the animal, of whom I spoke to you in my last lecture, a fourth time; the blood, received into this vessel, has not coagulated; but what is much more extraordinary is, that thirty centilitres of it have supplied the large mass of fibrine you see on this plate. I confess all this surprises me beyond measure; the more we bleed the animal the greater the volume of the fibrine becomes. The hypothesis I ventured on, as explicative of this most strange phenomenon, is far from having the solidity I could wish; indeed, the possibility of the fibrine of the blood being replaced by that of the muscles or organs generally becomes extremely questionable, when we reflect on the various points of distinction which I have, by experiment, shown to exist between that principle as contained in the blood and in muscular tissue. With regard to the animal on whom this gradual defibrination has been practised, his state deserves

consideration. He is so extremely weak that he lies constantly on one side; the cornea is dull; the contractions of the ventricle have lost their former energy, and there is marked anorexia. When the last venesection was practised he fainted, and the state of syncope lasted so long that it was supposed he was dead; however, a few moments after, respiration was re-established, and, some way or other, he continues to live on. I shall take care to acquaint you with the further results of the experiment.

In my last lecture I once again gave you an illustration of a fact simple in itself and easily cognisable, while it is also, through its consequences, of primary interest, and connected with every branch of pathology. I said, and proved what I said, that determinate causes deprived the blood of the faculty of coagulating; I showed that that liquid, when thus modified, transuded through the walls of the vessels, became effused into the parenchyma of the organs, and developed in them certain local affections, the production of which was, till of late, exclusively attributed to the solids. Was not this a most serious error,—this unvarying attempt to discover in the vitality of our tissues the cause of the majority of the disorders to which they are liable? I know you think so, for you have distinctly seen that in cases where the famous "*stimulus*," according to the prevalent doctrines, determines an afflux of blood to a part, there is in reality nothing more than a modification of the properties of the liquid, and an exemplification of the mode of action of one of the grand laws of physics, that of imbibition and exhibition, in organised membranes. But this lamentable ignorance of, or perhaps, more correctly speaking, contempt for, the principles of physical science, has been prolific of baneful consequences.

Look to the condition of physiology from its origin; sunk into a kind of forced subserviency to an exclusive system of vitalism, we find it encumbered with absurd reveries and startling conjectures, and lending an unsound support to ridiculous pathological theories; even at that early period appeared the famous dogma on which the homœopathists have since laid their hands and tortured to their purposes—*similia similibus curantur*. An organ is *inflamed*, an invisible cause (the *stimulus*) is said, by some mysterious process, to have attracted the blood to it; to have enabled that fluid to surmount the obstacles to its passage which it encounters in the *contractions* of the capillary vessels, and to distend those vessels to such an extent that they give way, while it forces itself through the rent. What, then, becomes of the *stimulus*? No one can tell us; none know how that subtle agent managed to slip into one of the capillary vessels; none saw it enter; none have witnessed its departure; and far be from me the silly pretension to tell you more about it than the bright geniuses who invented it. But this is not all; let us turn to the plan employed to prevent the imminent conflagration of the animal machine consequent on the intrusion of this terrible *stimulus*; it is based on the excellent axiom I have just quoted. As the *stimulus* has drawn the blood to a particular spot, and therein acted as an immaterial invisible sucker, why we will simply (say the irritationists) set in action against it a living material one, nay, fifty, a hundred, two hundred, or more of them; and the arch enemy will soon be forced, by this imposing apparatus of *absorbing mouths*, to quit the scene of his exploits! I really feel a sort of shame, Gentlemen, in alluding to these extravagant absurdities, but their influence on our science has been so powerful, that no opportunity should be lost of heaping on them the ridicule they deserve.

But to turn from these disagreeable passages in the history of physiology: I have already frequently spoken to you of the subcarbonate of soda, a salt which exists, in the normal state, in the blood, mingled with the other elements of that liquid. This fact would authorise us in presuming, nay, in affirming, that that salt is an innocent substance; and so it is under the circumstances stated, but under others its character is very different. Thus, if injected into the veins in large doses, it immediately causes death, and in this manner consequently possesses the properties of an energetic poison. Experimental inquiry has removed all doubt respecting its mode of action; it has shown that the aqueous solution of the salt will, if placed in contact with blood, prevent the coagulation of that fluid, and further demonstrated, by injections performed on living animals, that its effects are the same on blood when within, or removed from, the circulating system. This is a fact of the highest interest; for the subcarbonate of soda enters liberally into the composition of many medicaments, is in consequence frequently introduced into the economy, and may, when there, induce most serious disorders. For my own part, I will always watch over its employment with much solicitude, and henceforth be more cautious about prescribing it even in moderate doses. For this practical, and, I dare to say, valuable result, we are indebted to experimental research alone.

You are also acquainted with the action of putrid water; you have learned that a small quantity of it injected into the veins liquefies the blood; in other words, that it kills; however, an important distinction is to be established between the two substances as regards their action; while the subcarbonate of soda acts specially on the

lung, that organ is scarcely affected by the injection of putrid water into the veins, which, instead, exercises all its deleterious influence on the intestinal canal. Now water is not the sole vehicle of miasmatic particles, the fluid we breathe frequently holds such atoms in suspension; it is putrid exhalations that give rise to the intermittent fevers of marshy localities, from which the inhabitants sometimes succeed in escaping, by covering the face with a veil during the hours of sleep. The air, while traversing its tissue, is, as it were, sifted, and so reaches the organs of respiration purified of the vegetable or animal molecules with which it is loaded. None will be found at the present day to question the fact that the yellow fever is produced by the absorption of miasmata, arising from the putrefactive fermentation of marine animals and plants of all sorts left on the shore by the ebbing tide. Hence that affection is to be looked on as resulting from infection; and if it were desirable to have other facts referrible to this question, they might be easily had. Does not every pathologist, without exception, now recognise the fact, that the "typhus," which is known to attack armies, the crews of ships, and, in a word, large assemblages of men, under whatever circumstances they may be congregated, is communicated by infection? I was a close observer of that terrible scourge of our population in 1814; and even at that period I remarked a particular liquidity in the blood of the individuals attacked with it. This fact made so strong an impression on me, indeed, that I was in the habit of prognosticating a fatal termination, or the contrary, according to the degree of its fluidity. Many authors have noted the occurrence of this phenomenon in such affections.

As I have touched on the subject of disorders which it is the fashion to call *contagious*, I may be permitted to say a word or two on the most frightful of them all, the plague. In consequence of the progress of physics and chemistry certain medical questions have become much less obscure than they formerly were; we are now enabled to attack them on philosophical principles, and to investigate them calmly and deliberately; and, as respects the subject of the plague, the result of this improvement is, that convincing arguments pour in from all quarters, calculated to prove that that disease originates, and is propagated, not by contagion from man to man, not by contact with merchandise or contaminated substances, but by infection. The atmospheric and other conditions of Egypt, its ordinary cradle, are eminently favourable to its development. Torrents of rain deluge the land in an instant after long-continued drought; an intolerable degree of heat is as suddenly exchanged for an icy coldness; add to these rapid transitions the annual inundations of the Nile, which, as it retires from the ground it temporarily covered, leaves behind it multitudes of carcases of animals exposed to the incessant action of the scorching sun of Africa; consider, besides all this, the insalubrity of the habitations and the want of cleanliness of the Arabs, and you will not question the justness of my statement. Nor must the terrible hurricanes, which have buried ancient Egypt under mountains of sand, be forgotten; like messengers of death, they bear with them, and spread far and near, the putrid emanations gathered in their passage from place to place, as though it were their office, after having shaken the soil to its foundation, to exterminate the beings dwelling on it.

Gentlemen, the question of the mode of propagation of the plague is far from being an idle or unimportant one. Admit for a moment, and for argument's sake, that it is an infectious disorder,—set aside the bugbear of contagion; what good end would those puerile precautions, misnomered sanitary, then effect? Where would then be the utility of your quarantines, which shackle the freedom of commerce; of your lazarettos, where you disinfect what never was infected,—where you neutralise a virus, the existence of which is to this hour a problem? Who can affirm, on conviction, that the plague is transmissible in bales of goods? Where are the experimental proofs of the position? Where even a few well-established facts, calculated to give it plausibility? None are to be found; but instead we have, as usual, a profusion of assertions; rumours originating in the terrors of panic-struck populations; facts cited with emphasis by the vulgar, but contested by men of science. People who look only to the surface of things, cry aloud, when they hear so much as a doubt let fall concerning the contagion of the plague; but all the clamours in the world are mute before the sage. The captivity of Galileo has not prevented our planet from operating its revolution round the sun, nor the latter body from remaining motionless in the centre of its system. Several medical men, M. Chervin among others, have demanded an authorisation to devote themselves to the solution of the all-important question under consideration; to perform a series of experiments on contaminated goods, in an uninhabited island, far from the habitual foci of the plague; but Government has refused its permission, so deeply does it dread giving umbrage to absurd or interested prejudices.

The opinion I espouse is that held by persons whose position gives them the best opportunity for examining the question. Clot Bey, among the rest, is of opinion that the plague is not a whit more contagious than "typhus;" so that this question, too,

is referrible to that of substances capable of liquefying the blood. It so happened that, while engaged in one of my recent courses of lectures in explaining my doubts as to the contagious nature of the disease, I had among my auditors M. Lacheze, a medical man who had just returned from Egypt, where he had had extensive opportunities of observing it. After I had concluded my lecture he came up to me and informed me that, in truth, the blood of plague patients never coagulated, and was always clammy and blackish. "You have," he added, "hit upon the true view of the question; all those who have observed the plague closely, and uninfluenced either by apprehension or prejudice, will share your opinion respecting it, if they have not already done so." By-and-by, when we come to study the action of gases on the blood, I will not neglect to examine thoroughly the effects produced on that fluid by the carbonate of ammonia,—a salt, you are aware, generated by the putrefaction of animal matters. The importance of ascertaining what substances destroy the coagulability of the blood flows in the most direct manner from what I have been saying. So far I am only enabled to enumerate a small number; the list will, however, soon be enlarged, for I am about now to commence a series of experiments, wherein I propose to bring the different soluble salts, &c., into contact with the blood; at the same time we must endeavour to find out those capable of promoting coagulation, and so of serving, if I may be allowed the expression, as antagonists to the others. It is possible that our investigations may not lead to the expected results, but the trial is well worth making.

As the carbonate of soda and putrid water unquestionably act on the blood in glass vessels in the same manner as on that circulating in the living tubes, I shall simply, in order to avoid a useless sacrifice of animals, place blood in contact in glass vessels with the following substances:—

Sulphuric
Hydrochloric
Acetic
Oxalic
Tartaric
Lactic
} Acids.

1st Experiment.—Sulphuric acid.

Judging from medical practice and the notions generally admitted, we should fancy that this acid would augment the coagulability of the blood; in fact, it is employed internally to stop hæmorrhage, and to cauterise surfaces from which blood flows in undue quantity. Besides this, "sulphuric lemonade" is one of the most common drinks given in our hospitals to arrest internal hæmorrhage from the uterus, lungs, &c.; the acid is, consequently, looked on as promoting coagulation; but the truth is otherwise; here is a mixture of five centilitres of water, four drops of concentrated sulphuric acid, and one centilitre of blood. These substances have been in contact since yesterday, but the fibrine presents no appearance of coagulation; nay, more, the colouring matter is altered in its properties, as you see by its deep black colour. This action of the acid on the globules, I mean the removal of their normal colour, and that of their characteristic property of reddening when in contact with atmospheric air, is a further mischief in addition to the deleterious influence of this acid on coagulation; it is possible, therefore, that instead of putting a stop to hæmorrhage, liquids containing sulphuric acid promote the escape of the blood from its vessels.

2nd Experiment.—Hydrochloric acid.

Hydrochloric acid, in the same proportion, has also prevented coagulation.

3rd Experiment.—Acetic acid.

This acid is used with our food, and certain young ladies consume it to a large amount; in order to remove premature fulness of person they are in the daily habit of drinking a great quantity of it diluted with water, without reflecting on the future sufferings of which they thereby lay the foundation. Be this as it will, the blood before us has not coagulated; this property of acetic acid is well known to our pork-butchers, who are in the habit of using vinegar for preventing blood from clotting.

4th Experiment.—Oxalic acid.

Three grains of oxalic acid, dissolved in a little water, have completely liquefied the mixture; there is no trace to be seen either of globules or fibrine. This substance has, as you well know, long been classed among energetic poisons, but its most extraordinary character is that sometimes it acts as a corrosive on the coats of the stomach; sometimes as an irritant on the entire intestinal canal; sometimes, it is said, on the nervous system. These differences may possibly be dependent on its action on the blood.

5th Experiment.—Tartaric acid.

Tartaric acid is an essential constituent of almost all our wines; it also enters into the composition of tartar emetic. No coagulum has formed in the vessel in which the mixture has been made; but you see that there is a slight deposit at the bottom, which will require examination under the microscope.

6th Experiment.—Lactic acid.

This is not a powerful acid, nevertheless it has proved energetic enough to prevent coagulation.

We next come to a series of substances more or less *alkaline*; it will be interesting to establish their action on the blood.

1st Experiment.—Pure potassa.

Eight drops of pure potassa, in solution, have been added to the blood in this vessel; the mixture is of purplish hue, and no coagulation has taken place, unless we are to take into account these flabby clots resembling current jelly.

2nd Experiment.—Ammonia.

This alkali has dissolved the colouring matter; the whole is quite fluid.

3rd Experiment.—Lime-water.

The lime-water employed here has not dissolved the colouring matter; but it remains to be learned whether it has not otherwise modified its properties; here, again, there is no coagulation.

4th Experiment.—Hydrosulphate of Ammonia.

This salt has not, and the fact is a curious one, prevented coagulation from taking place. M. Bonnet, surgeon to the Hôtel Dieu at Lyons, has proved that this substance forms in pus when left in contact with the air, as is demonstrable by its action on a piece of paper previously steeped in a salt of lead. He has gone further than this, and stated that the absorption of the gas produces the symptoms ascribed to the absorption of pus; an assertion, however, far from being distinctly proved. Besides, the gas does not remain in the blood, but escapes by the respiratory passages. It has, in this vessel, formed a very soft and fœtid clot.

5th Experiment.—Nitrate of potassa.

This salt has also effectually prevented coagulation.

Hence, of all the substances with which we have mingled the blood, one alone slightly promoted its coagulation, and this the very one to which we should have least inclined, *à priori*, to ascribe such a power. But errors of this kind will always be committed when we indulge in hypothesis instead of basing our opinions on experiment.

I shall terminate this lecture by injecting oxalic acid into the jugular of a dog; the vein is laid bare, and a ligature fixed to its upper end to prevent hæmorrhage. I now introduce the point of a syringe into its lower end; the instrument contains 2 drachms of a solution of 2 grammes of oxalic acid in 50 centilitres of water; I shall, consequently, have injected about 35 cent. of the acid into the circulation of the animal. I push the piston, lest by an abrupt or jerking injection I should cause immediate death. The symptoms are, however, very slow in developing themselves, and the lateness of the hour obliges me to defer, for the present, prosecuting the experiment any further.

ON

ANTEVERSION AND ANTEFLEXION OF THE UTERUS.

By W. Hayle Walshe, *M.D.*, *&c.*

(Continued from page 514.)

Invasion and Symptoms.—It is next to impossible to fix the time of origin of the displacement with precision in cases where it appears to have been gradually effected: when a sudden occurrence, this difficulty is not encountered. Thus, in one instance, when anteversion followed a fall from bed, various symptoms of the affection were immediately felt; and several other similar cases are to be found in authors. Some circumstances serve to indicate that the anteversion took place on the 10th Nov., in the case I have reported; nevertheless, there are others, such as the absence of pain, and the patient's not having strained herself in any way at the time, which argue against that opinion. The subjects of the complaint have, in some instances, themselves fixed the period of its origin with tolerable accuracy; in one very interesting case the complaint had been gradually coming on for four years, until at length sudden and complete retention of urine supervened, lasted thirty hours (the patient objected to the use of the catheter), and was attended with pain in the loins, hips, and groins, and high fever. Generally speaking, the symptoms of invasion are the same as, though usually less marked than, those of the perfect displacement.

When anteversion is established, its most constant symptom is a troublesome sensation of fulness, or weight, sometimes amounting to dull pain in the pelvis, and very frequently accompanied with "bearing down." The loins, pubes, groins, and thighs, are also the seat of a pain of the dragging kind, which is increased by the erect attitude, by walking, and by carriage exercise, while it is usually relieved by the recumbent posture; cases, however, have been published in which lying down simply changed its position without lessening its severity.

The functions of the organs on which the displaced uterus presses are generally in a disordered state; there is either frequent desire to pass the urine, which act is accomplished with much difficulty, or defecation is exceedingly laborious, or both those symptoms are present; tenesmus, too, existed in some cases to a very distressing degree. These symptoms are, however, by no means of constant occurrence; in several instances no mention is made of them, and two patients, observed by Lagrand and Rayer, are distinctly stated to have excreted the contents of those organs with facility. It is to be supposed that the varieties in this class of symptoms must depend on the degree and

nature of the malposition, but no relation can be traced between them from the analysis of recorded cases. In my own case, in which it would seem fair to infer, more especially from the obliquity of the uterus, that its neck could not have borne with as great a degree of force on the rectum as its fundus on the bladder, the act of defecation only was interfered with. The excretion of the urine however is, in anteversion, the more frequently affected of the two functions. Pain during coition has been noted occasionally, even in cases where no distinct evidence of the existence of chronic metritis existed.

These are the chief symptoms which, in ordinary cases, result directly from the altered position of the uterus; but there are others of much importance, caused by the state of irritation its displacement almost invariably induces in the organ. Among these may be enumerated pain of more or less severity in the region of the sacrum; disordered menstruation, with leucorrhœal or sanguinolent discharges in the intervals; it is, indeed, very possible that the malposition may suffice in itself to produce these effects, but published cases do not prove the fact to be so. Leucorrhœa is nearly a constant symptom, even in cases where its existence cannot be ascribed to the presence of a pessary. I have not discovered any analogous occurrence, in other instances, to the flow of blood in M. Louis' patient; the discharge appears, indeed, to have been throughout more abundant in her case than is usually observed. M. Rayer informs us, that in one of his cases there was no particular discharge, but that the catamenia were always accompanied with clots. This remark involves a belief, that the occurrence of coagula in the menstrual fluid is abnormal, a fact which is so far from being fully established, that the diligent inquiries of Louis and others go to prove, on the contrary, that they are always formed during the menstrual periods of many women.

Among the rare consequences of the displacement are retention of the menses and sterility. The former will only occur when adhesions have formed between the os tincæ and posterior wall of the vagina; the latter need not be apprehended, provided the orifice can be brought towards the centre of the vagina.*

Impaired digestion occasionally occurs, and this is productive of constipation, which, as it tends to increase the displacement, stands at once in the relation of cause and effect to it.

These symptoms, which are subject to extreme variety in point of severity, are, however, only presumptive, not decisive of the existence of anteversion; its true signs are furnished by vaginal examination; the mobility and height of the organ, being variable, are unimportant points. The first point of importance we ascertain, is the absence of the orifice from its natural situation; this establishes the existence of displacement, most probably either anteversion or retroversion. By persevering for a short while we succeed (except in rare cases, where *elevation* coexists with the malposition in question) in reaching the neck, with its orifice turned towards the sacrum; and the evidence its position affords of the existence of anteversion is confirmed by finding a tumour at the anterior wall of the vagina, pressing on the neck of the bladder. The continuity of this tumour with the neck of the uterus, which is proved by gliding the finger along the lateral border of the organ, shows that it is formed by the fundus. In cases of anteflexion the uterine orifice will be found in the centre of the vagina, but the position of the fundus will be the same as in anteversion; and when flexion exists to any considerable amount, the point of the finger may be easily lodged in the angle formed by the two divisions of the organ; there is no particular sensibility in the parts, unless in subjects affected with metritis.

The information afforded by the speculum is by no means so accurate as that obtained by introducing the finger. When the symptoms of irritation are severe, however, it will be well to ascertain by its use the condition of the substance and mucous covering of the cervix. Examination per rectum will occasionally, as in one of the cases observed by Levret,* enable us to reach the neck of the organ, when it is next to impossible to make out its position per vaginam; but it will be rarely necessary to have recourse to this disagreeable aid in diagnosis.

Causes.—Authors enumerate a variety of causes for anteversion, but the majority of them seem to have been admitted rather on theoretical grounds than from experience. The age of individuals cannot be said to exercise any distinct influence on its production; for though the greater number of patients were between 35 and 45 years old, yet the mean age of four among them (not

* In a work on Impotence and Sterility, by M. Mondat (Paris, 1823), will be found a description of an instrument called the "metrastère," devised by that gentleman for remedying, by a temporary change of the position of the os tincæ, this serious complication of the complaint.

* Dr. F. Churchill (Op. Cit. p. 263) falls into a slight error in supposing that Levret met only with the single case in which the operation of cystotomy was unfortunately performed. On the contrary, his subsequent practice enabled him more than once to avail himself, successfully, of the knowledge then painfully acquired.

including Madame B.'s case) was 24. The agency of temperament is less questionable, as, with one exception, the patient is said (when the circumstance is noted at all) to have been of lymphatic or nervous habit. Meagre subjects are not proved to be particularly liable to the complaint.

On the relation of trades, sedentary or active, to anteversion, I have nothing positive to offer.

The power of frequent child-bearing in inducing anteversion is, in the estimation of writers incontestable. There is, probably, some exaggeration here, for of the eighteen patients whose cases are analysed in this notice, six had never had any children; in one of these (not Madame B.'s case) the hymen even existed;* three had had only one; one had two children; and four others five, six, or "several." A uterus which has never been in the gravid state is, therefore, liable to the displacement from the same direct causes as that of a woman who has been frequently pregnant. Again: it has been said that the occurrence of anteversion during pregnancy is impossible; a case, given by M. Dugès, in which the fundus fell between the adjacent walls of the vagina and bladder in the tenth or twelfth week of the state, and there are others of the same kind, shows the statement to be erroneous. Undue width of the vagina, in all probability, predisposes to anteversion.

The general health of the great majority of the patients was in no wise remarkable: one was subject to hysteria; one or two to very obstinate constipation; in the greater number, however, the catemenia were irregular in quantity and in the periods of their appearance,—certainly, I should think, in a greater proportion of cases than the usual mean number in a series of women taken without selection; two of them were subject, from an early age, to almost constant leucorrhœa. These disordered conditions of menstruation tend to induce engorgement, such as is frequently met with in the anteverted uterus, and is by some presumed to be its immediate cause. I have already stated that engorgement is not a constant appearance; and that, when it does exist, it is not limited to the fundus or anterior wall. Besides, as in the instance of M. Louis' patient, it is often very difficult to satisfy oneself whether the hypertrophous state preceded or followed the displacement. It is, perhaps, most likely that, as Desormeaux suggests, its influence may be really confined to increasing a state of anterior obliquity to anteversion.

* I am aware that the existence of the hymen is far from being conclusive of the virginity of its possessor. I recollect to have seen in M. Ricord's wards, at Paris, a female in whom the hymen was perfect, though she had borne a full-grown child.

The direct causes of the displacement are, like those of luxations, of a mechanical nature; these are, among others, falls, violent efforts with twisting of the abdomen, straining at stool, vomiting (Chopart), jolting in a carriage, &c.

Slow displacement may be effected by the growth of tumours, either situate behind the organ or attached to itself; the enormous size of the liver and spleen may have contributed to its production in Morgagni's patient, as suggested by M. Ammeline.

Diagnosis.—Examination per vaginam is the true source of correct diagnosis in this affection, and the tolerably full description I have given of the conditions it discloses renders it useless to dwell on them here. Were it the habit of medical practitioners to have recourse to this method of examination at an earlier stage of uterine diseases than is usual, I feel persuaded the number of cases treated as simple inflammation of the cervix or body of the uterus, would be less than it now is. In truth, there is nothing in the *rational* symptomatology of those affections to distinguish them from the malposition in question, especially when this is slightly developed, and, consequently, at a period of its existence when judicious treatment is most likely to be permanently beneficial.

The distinction of anteversion and anteflexion from retroversion and retroflexion, can only be satisfactorily established, in the present state of our knowledge, by making out the position of each part of the organ with the finger; though the condition of the patient, with respect to pregnancy and parturition, may serve as a guide, sometimes, to the differential diagnosis.

Prognosis.—Unlike the converse displacement, retroversion, anteversion does not appear, in any instance, hitherto met with, to have terminated fatally, unless we except the case I have myself described. Here the effusion of blood, the evident cause of the peritonitis, was probably an effect of the unnatural position of the uterus, though I confess it is difficult to prove the fact.

Independently of its own symptoms, those of the chronic metritis which usually attends it, render it, in general, a most troublesome affection; but, at the same time, it may certainly exist, in a slight degree, without causing serious distress. This is, indeed, true of all uterine malpositions. When the cervix contracts adhesions with the posterior wall of the vagina, the womb is irreducible.

Desormeaux conjectures that, inasmuch as the vitality of the uterus diminishes at the natural period of the cessation of the catamenia, the patient then labouring under anteversion, may lose all consciousness of its symptoms. It is worthy of note that the only case I have met with, by which the

correctness of this hypothesis could be tested, directly disproves it.

Treatment.—This will vary according to the length of time the complaint has existed when medical advice is sought, and according to the severity of the symptoms. Let me premise, however, with a few words on what may be termed the natural cure; I allude to the change occasionally effected in the position of the cervix by the advancement of pregnancy, a change which seems to have been followed by reposition in one case, at least, observed by Madame Boivin. In the second month of pregnancy the patient felt various symptoms that were found to depend on anteversion; the neck was so high up in the pelvis that it could not be reached even with the instrument to be described presently. No particular treatment was adopted, but in the fifth month the cervix descended to within reach of the finger. We are not informed whether a relapse occurred after parturition.

The treatment is either palliative or curative; the former is the only one that can be resorted to in subjects of excessively irritable constitution, and must, in all cases of engorgement, be employed before trying the mechanical modes of effecting a permanent cure.

Reposition may, however, always be attempted, unless when the tenderness of the parts is extreme. Simply lying down will sometimes restore the organ to its natural position; if it fail to do so, the practitioner must employ the taxis. Having first placed his patient in the supine posture, with her knees raised, he lifts up the fundus with one or two fingers of the right hand, steadying it in its new position by pressing with the left hand on the hypogastrium; this done, he moves the right index finger backwards and upwards, passes it behind the cervix, and draws that part downwards to the centre of the vagina. If the neck be beyond reach of the finger Madame Boivin's instrument may be advantageously employed.

As the accompaning figure shows, it resembles one branch of a forceps, being about six inches long, and curved in the form of an italic *f*. The blade portion is introduced into the vagina, with its convexity looking backwards, and the cervix seized in the fenestra; the depression of that part is then easily effected. The means of retaining it *in situ*, when art is necessary for the purpose, will be spoken of presently.

Cupping on the loins, leeching the perineum, or cervix uteri, the use of emollient vaginal injections, and of the hip-bath, and persistence in the recumbent posture, are indicated when symptoms of inflammatory character are present; they are curative as regards uterine inflammation, palliative as regards the anteversion.* I am enabled, from experience, to speak very strongly of

Fig. 2.

the excellent effects of leeching the perineum, when it is the object to relieve that very ordinary symptom of uterine disorders, bearing down pain at the anus. But, if there be decided inflammation of the cervix, the direct application of leeches to the part is incomparably better practice than abstracting blood by any other means, or in any other situation whatsoever. The very great number of instances in which I saw it successfully employed in the Parisian hospitals, leave no doubt of this important point on my mind.

All exertion should, of course, be avoided for a time, even such employment as would require the patient to sit up in bed or on the sofa, and defecation rendered as easy a process as possible by the use of laxatives. Astringent injections, such as solutions of the sulphates of zinc or copper, infusion or decoction of plants containing tannin, or that principle itself in solution, especially where there is discharge of blood, may be

* Some practitioners are of opinion that the hip-bath, in consequence of its favouring the afflux of the fluids to the pelvis, rather increases than lessens irritation in uterine disorders. So far as I am aware, however, this view, though specious enough, has not been sanctioned by experience.

used after the removal of all inflammatory symptoms. These means will diminish the habitual leucorrhœa, restore its lost tone to the vagina, and when the continuance of the displacement is the effect of deficient tonicity, may, in fortunate instances, complete the cure. The cold bath, sponging the parts with cold water, and, better still, sea bathing, will conduce materially to this favourable result.

The curative treatment consists in the application of sponges or the pessary after reposition. The former are ineffectual, because they rather raise the organ up, than act so as to maintain it in its natural, nearly vertical position, and because they dilate the vagina by swelling, from the absorption of the secretions of the part. They require to be very frequently removed and cleansed, nor can they be employed while the catamenia are present.

The utility of the pessary is less contestable, but the best form to give it in this affection is not so clear. The form originally recommended by Levret was the *stem-pessary*, and this has been further spoken of by Désormeaux, as the only kind calculated to fulfil the end proposed. It is true we can hardly fancy, *a priori*, that the bung, or ring pessaries, will suffice to keep the uterus straight, whereas the stem pessary receives the os tincæ in its excavated portion, or cup (the peculiarity of the *pessaire à bilboquet* is imitated in its construction), fixes it well in the centre of the pelvis, and so keeps the fundus erect; and, as has been further urged in favour of the latter instrument, the stem being tightly embraced by the vagina is not liable to shift from its place, as is the case with the other kinds. But notwithstanding these speculative objections, the efficacy of the bung pessary has been proved by experience; it may, therefore, be first given a trial. The time the instrument should be worn to effect a cure is calculated by Levret at about twelve or fifteen months; Désormeaux considers that a shorter period will be enough in many cases.

It is evident that in cases of pure anteflexion, where the os tincæ is already in the centre of the vagina, the employment of pessaries, such as are now known, can be of no avail; nor is much greater success to be expected from its use in subjects affected with the mixed kind of displacement observed in M. Louis' patient, as, from the flexibility of the organ at its angle of flexion, the traction of the neck downwards and forwards, will have little or no effect on the fundus. In such cases local palliatives and constitutional treatment will be our chief resource; the improvement in M. Louis' patient shows how much mere rest and quiet will effect in subjects belonging to the working classes. Wearing a sponge placed as high as possible between the body of the uterus and bladder would perhaps be worth a trial, and Désormeaux has proposed ("Dict. de Méd.") a new description of pessary for these cases; but as he had had no experience of its efficacy, I think it enough to refer for its description to his article in the above work.

The number of objections urged against the use of pessaries in general by some eminent men, among others, Professors Hamilton and Dieffenbach, made me anxious to ascertain their correctness as regards anteversion. The number of cases I have been able to discover are, however, evidently insufficient for the decision of so practically important a question; still, it may be well to state that the general impression derived from them is, that the evils of pessaries have been much magnified. The increase of the habitual leucorrhœa, which no doubt occurs when they are first worn, gradually disappears; and to it Levret and Désormeaux are inclined—but this is mere hypothesis—to attribute partly the disengorgement of the organ. It is right to add that the pessary is by no means an infallible remedy in the complaint; it may not only fail in curing it, but scarcely act as a palliative.

For the fullest and best digest of the state of our knowledge respecting the use of the pessary, with which I am acquainted, I beg to refer the reader to the work of Dr. F. Churchill.

The serious complication of retention of the menses, already adverted to, was boldly and successfully combatted by Gauthier, by making an incision about two inches long, in the antero-inferior part of the uterus, from right to left. The operation was followed by the evacuation of four pints of menstrual fluid, and instant relief of several bad symptoms that had arisen. The catamenia regularly passed afterwards through the artificial opening, and the same route was taken (so we are allowed to suppose at least) by three infants, of whom the patient was afterwards safely delivered.

It does not enter into my plan to consider the treatment of cases in which the anteversion, converted into anterior obliquity, persists up to the moment of delivery in pregnant women. Valuable information on the subject will be found in Professor Hamilton's "Practical Observations."

RUPTURE OF THE STOMACH.

RELIEF OF THE PAIN BY OPIUM.

To the Editor of THE LANCET.

SIR:—If you consider the following case worthy of a place in your valuable Journal, I shall feel obliged by its insertion, and remain, your obedient servant,

WM. EDWARD CROWFOOT.

Beccles, Suffolk, Dec. 24, 1838.

E. W., aged 36, of a scrofulous diathesis, had suffered for many years from occasional pains in the stomach, accompanied by a very constipated state of the bowels, and the ordinary symptoms of severe dyspepsia. In the month of June, 1838, she was suddenly attacked with profuse hæmatemesis, which was arrested by the usual treatment. At the end of a fortnight she returned to her ordinary employment as a lady's-maid, and applied no more for medical assistance until the 2nd of December, 1838, when she was suddenly seized, at 10 A.M., with most excruciating pain in the left hypochondrium, extending across the epigastrium up to the point of the left shoulder, and down to the left groin. The pain was so severe that she was incapable of speaking, or of accurately describing its situation. The pulse did not exceed 60 beats in the minute; the tongue was perfectly clean; the skin healthy, and the bowels had been slightly relieved in the earlier part of the morning. Upon making inquiry amongst her fellow-servants it did not appear that anything deleterious had been swallowed; she had suffered a little pain on the preceding evening, leading her to apprehend one of her usual stomach attacks. She had passed rather a restless night, but arose at an early hour, and ate her breakfast with a good appetite. Eight ounces of blood were taken from the arm, and an active dose of aperient medicine was given, which was instantly rejected. I then gave a pill containing calomel and opium, and directed the administration of a large enema. At the end of two hours I saw her again; she had taken another dose of aperient medicine, which had been retained, but produced no effect. The pain continued unabated, and the body was becoming most exquisitely tender under the ribs on the left side. A mustard cataplasm was applied over the painful part, and the aperient medicine and enema were repeated, but the former was rejected immediately, and the latter returned without producing any feculent evacuation; the pain came more in fits, somewhat resembling labour-pains, and there were intervals of comparative ease, but the side continued exquisitely tender, and the slightest quantity of food taken into the stomach produced the most agonising pains shooting down to the groin and up to the shoulder; twelve leeches were ordered to be applied to the side, and one grain of opium with two of calomel, to be taken every hour.

At 9 P.M. she continued as nearly as possible in the same state, the body had become very hard, tumid, and rigid, and the countenance drawn and sunken, but the tongue was clean, and the pulse quiet, not above 80; although the blood drawn in the morning was neither cupped nor sizy, I felt it right to bleed her again to 18 ounces, and continued the use of the calomel and opium.

Dec. 3, at 8 A.M., I was called to her, and found her sitting on the close stool, endeavouring to pass her water, but without being able to do so. The bowels had been well relieved at about 5 A.M.; the evacuation was large, loose, and bilious; she was now extremely faint; pulse scarcely to be felt, and suffering much from the want to pass the water; she had had a restless night, but the pain was much relieved, although the body continued extremely tender; the urine was immediately drawn off, but was not high-coloured, and the blood drawn on the preceding night consisted of a large proportion of serum. She had taken four doses of opium, and was now ordered a little thin arrow-root, with a teaspoonful of brandy in it, which she liked much, and took without its occasioning pain, as all food had done on the previous day.

At 11 A.M. I found her much more comfortable, she had had some quiet and easy sleep, and spake cheerfully about her speedy recovery; but the body still continued swollen and tender. At about 1 o'clock the breathing and pulse became quick and hurried. At 3 P.M. cold perspirations supervened; and at 4 P.M. she expired, continuing perfectly sensible to the last, suffering but little pain except when moved, but much troubled during the last few hours with flatulent eructations.

Examination of the Body eighteen hours after Death.

Externally the abdomen was hard and swollen; upon dividing the peritoneum several pints of fluid escaped from the abdominal cavity, containing portions of coagulable lymph floating in it; the peritoneum was universally inflamed, and recent adhesions attached the contiguous folds of the intestines to each other; immediately under the edge of the left lobe of the liver the peritoneum covering the cardiac extremity of the stomach was in a state of active inflammation and ulceration, so that the contents of the stomach escaped through several pinhole openings. Upon dividing the stomach, the mucous membrane lining it appeared perfectly healthy, but about a finger's breadth from the pylorus, in the under surface, and near the lesser curvature of the stomach, was a circular foramen about the size of a sixpence, having its edges smooth and rounded; this opened into a cavity almost equal to the stomach in size, lined with a membrane resembling the mucous membrane, and situated between the serous and muscular coats of the stomach; and this cavity it was in which the ulceration had taken place, and through which the contents of the stomach had been discharged into the cavity of the abdomen. Thus, it would appear probable that ulceration through the mucous and muscular coats of the stomach took place in June, when the patient suf-

fered the attack of hæmatemesis, that a cavity was gradually formed between these and the peritoneal coat, and the ulceration of the latter was the immediate cause of death.

Remarks.—This case is interesting inasmuch as it exemplifies the power which opium possesses of alleviating the agonising pain of so fatal a disease,—a relief, the value of which those only can appreciate whose painful duty it has been to witness the intense sufferings of a patient under similar circumstances. I cannot quit this subject without quoting the admirable diagnostic marks of this disease, as stated by Mr. Travers, in his interesting paper on this subject in the 8th Vol. of the "Med. Chir. Transactions:"—

"1. Sudden, most acute, and unremitting pain, radiating from the scrobiculus cordis, or the navel, to the circumference of the trunk, and even to the limbs; I may add, a peculiar pain, though I know not how to describe the peculiarity. Its intensity, like that of parturition, absorbs the whole mind of the patient, who, within an hour from the enjoyment of perfect health, expresses his serious and decided conviction that if the pain be not speedily alleviated he must die.

"2. Coeval with the attack of pain, remarkable rigidity and hardness of the body from a fixed and spastic contraction of the abdominal muscles.

"A natural pulse for some hours, until the symptoms are merged in those of acute peritonitis, and its fatal termination in the adhesive stage."

CASES OF

PUERPERAL MANIA,

WITH A DISSECTION AND REMARKS.

To the Editor of THE LANCET.

SIR:—I take the liberty of enclosing to you two cases of puerperal mania which came under my care in dispensary practice, and as I was able to make a post-mortem examination of one of them, knowing that the dissections of those dying under this disease are very few, I trust that they, and the few remarks which I have made, may be deemed worthy of a place in your Journal.

I remain, Sir, your humble servant,

MICHAEL J. MAC CORMACK, M.D.,
Physician to the Mullinahore Dispensary, County Tipperary.

Mullinahore, Callaw, Dec. 29, 1838.

CASE 1.—Ellen M——, æt. 19, married, was attacked on the evening of March 30, 1838, with a numbness in her legs, followed by pain in the head during the night, and was so delirious that restraint was required. Has been a fortnight delivered of her first child; the labour was natural, and was accomplished in three hours from the time the os uteri became fully dilated. She went on very well in her recovery in the usual manner, when the husband got a serious beating in returning from a fair, by which she was thrown into a state of great nervous excitement. In a week from this time, she was sitting one evening with her husband (March 30th), when she complained of great numbness in her legs, shivering, and pain in her head. She was greatly alarmed, and immediately ordered that I should be sent for. On my arrival I found her relations around her, in the greatest alarm, and the patient talking very incoherently, and giving directions for her funeral, and for other matters to be done after her death. I immediately shaved her head, applied cold lotions to it continually; mustard cataplasms to the legs and feet; cold drinks; to be kept very quiet.—Dover's powder, five grains, to be taken immediately, and repeated every three hours, if sleep be not produced.

March 31. Saw her this morning. The violent delirium has ceased, but she still continues to rave; total insomnia; skin dry; head very hot; pulse 120. I applied eight leeches to each temple, and ordered ten grains of Dover's powder at once, and to be repeated every six hours, until sleep be procured; fomentations to the thighs; poultices to legs and feet, which were severely blistered by the mustard cataplasm, also to a small sloughing ulcer on the back.

April 1. Has taken twenty grains of Dover's powder, without its producing any sleep; heat of head less: slight delirium still continues.—Five grains of Dover's powder every hour; eight leeches to each temple.

From the 1st to the 8th still going on much in the same way; no decided improvement; the same remedies continued. Being otherwise occupied I did not make any notes of the case until,

April 8. Has taken better than forty grains of Dover's powder, without any sleep being produced; is quieter, but still delirious; I ordered her the following powder:—

James's powder, eight grains;
Dover's powder, three grains;
Aromatic powder, two grains. Take the powder every hour; repeat fomentations.

9. Has had no sleep as yet; delirium of a quieter nature; appetite good; pulse 100; I found it advisable, on account of the long-continued insomnia, to give my patient watery extract of opium, two grains; effervescing draught.

10. Slept very well last night; delirium becoming less; pulse 90; bowels not moved for several days. Ordered her purgative draught, two ounces, at once, and to have it repeated in six hours, if the first had no

effect; continue the opium pill and effervescing draught; foot-bath at bed-time.

11. Slept well, delirium nearly gone. Omit pill.

12. Pulse 120; bowels still confined; had no sleep last night.—Purgative draught, two ounces, at once; one ounce until bowels are opened; repeat pill at evening.

13. Delirium gone; slept well. Ordered her some chicken-broth to-day. Omit pill and mixture.

14. Slept well without any pill.

15. Delirium gone; took one pill, gr. j.; slept well; pulse 86; delirium has not returned since the 13th. I this day discontinued my visits, as the patient was now perfectly well. I am sorry to say that in the next case I was not so fortunate in the result. The reasons I will give at the end.

Case 2.—Ellen Green, æt. 26, married, July 30th. Eight days ago she was delivered of a child, which she thought was a month before its time. Was bled by some horse-doctor, and took some medicine, ordered by the midwife, previous to my seeing her. She went out two or three days after her delivery, and took a glass of the *cratur* (*alias* whiskey) on the day after. She became delirious on the next day. Has now no lochial discharge, nor milk in the breasts.

31. Tongue foul, dry, and slightly furred. Is in a constant state of agitation, raving incessantly, and crying aloud, sometimes singing in a most plaintive manner. On questioning the nurse I found she had passed no urine since yesterday. A catheter was introduced, which drew off three pints of a high-coloured muddy urine. I was obliged to tie her hands, and confine her with something like a straight-waistcoat. Eyes not at all suffused, pupils natural; would wish to bite any one coming near her; pulse 120, very quick; one cheek more suffused than the other; temporal arteries do not pulsate violently, nor is there much heat of scalp; *no tympanitis*. In this case I also shaved the hair. Ordered cold vinegar and water to the head:—

Water, half an ounce;
Tartar. antimony, quarter of a grain;
Syrup, one drachm. A draught to be taken every hour.

August 1. Refused her medicine, but the nurse contrived to give it to her in the whey; has taken six grains of tartar emetic, it slightly affected her stomach; bowels not moved; has been quiet at intervals; broke her bonds yesterday; *slept none*; face much more flushed; eyes not suffused; belly not tympanitic. Contin. tartar emetic. Ordered her an injection of *starch*, ℥iv, and gtt. v of *black drop*, every second hour.

2. Injections not retained. Has taken since yesterday four grains of tartar emetic, and three grains of acetate of morphia, which I thought it advisable to order on account of the continued insomnia; was most violent during the night; has got a little sleep now for the first time; bowels obstinately constipated.—Opening draught.

3. Pulse 130, soft, small; no sleep; much more violent than yesterday, but when I saw her this morning she appeared quieter and very weak; skin cool; face not so much flushed; wandering and mental delusions still continue; I asked her to put out her tongue, which she did, and answered me reasonably; could not get her to swallow her medicines, and was obliged to force some down her throat, which did not operate, neither did the enemas. Gave her two drops of croton oil, which produced several copious evacuations; tongue moist and furred; bathed in perspiration when tied. Ordered *acetate of morphia*, half a grain, every third hour.

4. Died this morning at six o'clock. My assistant, who sat up with her during the night before, reported that she had slept a little last night; seemed quiet; then awoke raving with violent agitation, delirium, no convulsions or coma. After the bowels were opened last night, there was no perspiration; did not seem to suffer much agony, and died, as it seemed, *of exhaustion*.

Remarks.—This case I consider to be very interesting in many points. I had an opportunity of examining the patient a few hours after death, and will give the appearances which the brain and uterus presented. The pulse was quick from the beginning to the termination, which is contrary to the usual symptoms, or what the generality of authors on this disease describe. Those cases in which the pulse was quick and small, I have found almost always to terminate fatally: I speak from the experience of six cases which I saw during my hospital attendance, and all of which terminated fatally, the pulse averaging from 120 to 130. I think I shall be supported in this opinion, also, by those who have had more opportunities of seeing such cases; I am further confirmed in this point by my former master, Dr. Graves, of Dublin, from whom I first heard the remark. The day before this patient died I ordered her half a grain of acetate of morphia every third hour, as I find *it* the most valuable medicine in such cases for procuring sleep, as you can push it further without affecting the brain than any other soporific. That quantity, after being taken three or four times, produced sleep, and I was flattered by the hope that she would ultimately do well; but she became again delirious, which I generally find to be an almost fatal symptom. She made violent attempts to disengage herself from the straight-waistcoat; she seemed to die from exhaustion, and not from any congestion of the brain, which is another peculiar feature in this case, as congestion is believed by a great authority, and described by Burns in his valuable "Treatise on Midwifery," to

exist in the vessels of the brain in those who die of this disease. We find, also, that she had neither convulsions nor coma. Congestion did not exist; that fact was borne out by the *sectio cadaveris*, six hours after death.

Sectio Cadaveris.

The brain was perfectly healthy, both in its substance and its membranes. This is not the case, according to Burns' account; but this is the second case in which I found the brain and its membranes perfectly healthy. The latter was a patient of Dr. Graves while I was attending Sir Patrick Dun's Hospital, in 1837, and was in a great number of features like the case No. 2; and as this will perhaps reach the eyes of the worthy Doctor under whom I had the happiness and profit to attend, I may here say that my practice in this case was copied entirely from the treatment which he pursued in the case I now allude to, and of which I fortunately had notes. On examination of the brain and membranes in Dr. G.'s case, there was found neither congestion nor any other morbid appearances in the brain, and as the Doctor in that case said that in such form of puerperal mania there was no congestion, I am glad that I had an opportunity of producing another case in favour of that opinion. In my case there was no effusion into the ventricles, no thickening of the membranes, no subarachnoid effusion.

On examining the uterus, I found it of the usual size which it presents after delivery. It was slightly vascular, and the place where the placenta was attached was very evident. On account of the antipathy which the common people have to such examinations, I was not able to make a minute examination into any other organs; but as there are very few dissections of women dying of this disease, I hope that my present effort may be of some public advantage.

In conclusion, I wish to make a few remarks as to my opinion of the cause of death in the last case, and not in the first.

I am confident that I owed my success in the first case to not having abstracted blood generally, but locally; and from former cases, I hesitate not in saying that, in my opinion, it was from the woman having been bled previous to my seeing her that I was afforded so little chance of saving her. I am strengthened in this opinion from having seen the fatal termination of the case of Dr. Graves, whose patient was bled by the surgeon of the regiment (she was a soldier's wife) previous to her admission. It will not be out of place for me to quote here a few lines from a lecture of Dr. G. on this case, which I took down in my note-book:—He said, "In the treatment of this case there were two faults committed. The bleeding this woman was irrational, as experience afterwards proved. By all authorities this is laid down as unwarrantable where there is no inflammation," which did not exist in either his case or mine. I I think there is, therefore, little doubt but that bleeding was prejudical in the last case.

THE ALLEGED INCREASE OF SMALL-POX.—SUPPOSED FAILURES OF VACCINATION.

To the Editor of THE LANCET.

SIR:—In the speech of Dr. Gregory, at the meeting of the Royal Medico-Chirurgical Society, held Dec. 11th, 1838, as reported in THE LANCET of to-day, considerable, and, it appears to me, undue stress is laid upon the fact, that a larger number of cases of small-pox had been admitted into the Small-Pox Hospital during the current year, than in the year 1781, when, as in the year 1838, that disease was remarkably prevalent and fatal.

The increase stated by Dr. Gregory is, 35 out of 681 cases, or rather more than five per cent.; the marvel being that, in 1781, all the patients were unprotected either by inoculation or vaccination; of the patients received this year, 281 had been vaccinated.

That small-pox has been a prevailing epidemic during the present year; that it has been so truly an *approbrium medicorum* as to kill five and twenty per cent. of those attacked by it; that there has been a trifling numerical increase of the cases admitted into the hospital to which Dr. Gregory is physician during the present year, as compared with those admitted during the year 1781, both years being alike remarkable for the prevalence and fatality of this disease, I can have no difficulty in admitting; but that any inference is to be deduced from these facts, to the disparagement of vaccination, or in support of the notion that small-pox is more prevalent, or even so prevalent in 1838 as it was in 1781, I am at a loss to discover.

Perhaps Dr. Gregory will do your readers, —very many of whom are non-professional, and apt, therefore, to lean upon the authority of a name in professional matters—the kindness to place in parallels the numerical population of London, and the suburban districts from which the Small-Pox Hospital has received its patients, in the corresponding years 1781 and 1838. Has it not *doubled*, nay, *trebled* itself in the interval? And is this very vast disproportion between London in 1781 and London in 1838 to be left altogether out of the question, when instituting a comparison between the numbers of cases of a given disease from the same locality, so changed in the density and spread of its population in those two years? Instead of inquiring with alarm, an alarm, too, that propagates itself by contagion,— Why is small-pox as prevalent and as fatal in 1838

as it was in 1781? the inquiry, I submit, should rather be,—"To what agency are we indebted for its diminished prevalence, and seeing that we have succeeded in diminishing its fatality, how may its prevalence be still further diminished?"

Perhaps Dr. Gregory will also kindly inform his professional brethren at what period of the vaccine virus he is in the habit of sanctioning its transfer to the subjects who are brought to the Small-Pox Hospital for vaccination? Treading, though far off, and with unequal pace, in the footsteps of the immortal Jenner, I am satisfied "that," to adopt the language of that truly great man, "the operations of Nature are generally uniform, and that it is not probable the human constitution (having undergone the cow-pox) should in some instances be perfectly shielded from the small-pox, and in many others remain unprotected."

If vaccination fail in numerous instances, to what cause is the failure to be ascribed? If men would read the histories of diseases, and of their remedies, with greater attention, they would be at no loss for an answer in the present case. If the vaccination of the present day only modify small-pox in a multitude of cases, the vaccination practised by Jenner removed its objects out of reach of small-pox altogether. Vaccination, in the present day, would not have been recognised as vaccination at all in the days of Jenner. And the disease produced by our degenerate practice is not the *true*, but the *spurious* cow-pox, so beautifully described by the illustrious discoverer himself, and against which he so faithfully warned us beforehand by his writings.

To remind the profession of *his* example, at a time when that example is well-nigh forgotten; to win professional men back into the way which *he* paved for them, but from which very many vaccinators have widely wandered, will, I am sure, be the best apology I can offer for asking a reprint in THE LANCET of the following admirable passages from his Pamphlet, "on the Origin of the Vaccine Inoculation," 4to., 1801. pp. 6, 7, 8.

"In the course of the investigation of this subject, which, like all others of a complex and intricate nature, presented many difficulties, I found that some of those *who seemed to have undergone the cow-pox*, nevertheless, on inoculation with the small-pox, felt its influence just the same as if no disease had been communicated to them by the cow. This occurrence led me to inquire, among the medical practitioners in the country around me, who all agreed in this sentiment, that the cow-pox was not to be relied upon as a certain preventive of the small-pox. This for a while damped, but did not extinguish my ardour; for, as I proceeded, I had the satisfaction to learn that the cow was subject to some varieties of spontaneous eruptions upon her teats; that they were all capable of communicating sores to the hands of the milkers, and that, whatever sore was derived from the animal, was called in the dairy the cow-pox. Thus I surmounted a great obstacle, and, in consequence, was led to form a distinction between these diseases, one of which only I have denominated the *true*, the others the *spurious*, cow-pox, as they possess no specific power over the constitution.

"This impediment to my progress was not long removed before another, of far greater magnitude in its appearances, started up. There were not wanting instances to prove that, when the true cow-pox broke out among the cattle at a dairy, a person who had milked an infected animal, and had thereby apparently gone through the disease, in common with others, was liable to receive the small-pox afterwards. This, like the former obstacle, gave a painful check to my fond and aspiring hopes; but reflecting that the operations of Nature are generally uniform, and that it was not probable the human constitution (having undergone the cow-pox) should, in some instances, be perfectly shielded from the small-pox, and in many others remain unprotected, I resumed my labours with redoubled ardour. The result was fortunate, for I now discovered that the virus of cow-pox was liable to undergo progressive changes, from the same causes precisely as that of small-pox, and that when it was applied to the human skin in its degenerated state, it would produce the ulcerative effects in as great a degree as when it was not decomposed, and sometimes far greater; but, having lost *its specific properties*, it was incapable of producing that change upon the human frame which is requisite to render it unsusceptible of the variolous contagion, so that it became evident a person might milk a cow one day, and having caught the disease, be for ever secure; while another person, milking the same cow the next day, might feel the influence of the virus in such a way as to produce a sore or sores, and, in consequence of this, might experience an indisposition to a considerable extent; yet, as has been observed, the specific quality being lost, the constitution would receive no peculiar impression.

"Here the close analogy between the virus of small-pox and of cow-pox becomes remarkably conspicuous, since the former, when taken from a recent pustule, and immediately used, gives the perfect small-pox to the person on whom it is inoculated; but when taken in a far advanced stage of the disease, or when (although taken early), previous to its insertion, it be exposed to such agents as, according to the established laws of Nature, cause its decomposition, it can no longer be relied on as effectual. *This observation will fully explain the source*

of those errors which have been committed by many inoculators of the cow-pox. Conceiving the whole process to be so extremely simple as not to admit of a mistake, they have been heedless about the state of the vaccine virus, and finding it limpid, as part of it will be, even in an advanced stage of the pustule, when the greater portion has been converted into a scab, they have felt an improper confidence, and sometimes mistaken a spurious pustule, which the vaccine fluid in this state is capable of exciting, for that which possesses the perfect character."

Every student of the profession, before he attempts to vaccinate at all, should "read, mark, learn, and inwardly digest" the concluding paragraph, in particular, of the above quotation; and every member of the profession may, with safety to his patients, and satisfaction to himself, follow where Jenner led. I am, Sir, your obedient, humble servant,

WILLIAM BARRETT MARSHALL, R.N.,
Medical Officer of South Chelsea.

6, Cheyne-row, Chelsea,
Dec. 22, 1838.

TREATMENT OF FRACTURES
BY
THE IMMOVEABLE APPARATUS.

"What is honour?—A word. What is in that word? Honour.—What is that honour?—Air; a trim reckoning? Who hath it?—He that died o'Wednesday. Doth he feel it?—No. Doth he hear it?—No. Is it insensible then?—Yea, to the dead. But will it not live with the living?—No. Why?—Detraction will not suffer it; therefore I'll none of it: honour is a mere escutcheon, and so ends my catechism."

To the Editor of THE LANCET.

SIR:—Many months have elapsed since I had the honour of directing your attention to the fact of the *newly-resuscitated treatment for fractures* having been employed in this country upwards of three-quarters of a century. Although the occasional remarks which you have made on the subject do not appear to me to be so full or complete as the question deserves, yet I should not have troubled you with the present communication, did I not believe that the embryo importance of the plan (if I may be allowed the expression) which, I doubt not, will be evidenced by a more general adoption of this treatment, demands a full and explicit account of its English origin. The honour of its introduction into general practice in Great Britain undoubtedly belongs to Cheselden; and unless those who now so recently claim it as their own can confute published words, I insist that the merit of its scientific employment belongs to him alone. Cheselden's valuable labours, his pecuniary sacrifices, the simplicity and accuracy of his surgical practice, the fierce acrimony with which his scientific *innovations* (as they were then called) were attacked during his lifetime, should make us grateful to his memory, as one of those choice spirits who, like a good father, has left us the patrimony of an honoured name, whose posthumous fame it is our duty, if attacked, to defend. The reputations of such men are national property, and a right tacitly admitted, if not legally confirmed, in all civilised communities, must not be infringed without remonstrance.

In THE LANCET of April 28th is the following notice relative to this subject:—"The French and Belgian surgeons are disputing the honour of having invented the 'immoveable apparatus,' and the 'starched bandages,' for the treatment of fractures; but the same mode of treatment was communicated more than fifty years ago by a quack in London to a distinguished lithotomist of this country." In juxtaposition with the above remarks are others, "On the Treatment of Bronchitis." Some time ago Dr. Williams published a paper, in which the propriety of abstaining altogether from drink was inculcated as a mode of treatment of bronchitis. In a recent number of a French journal we read a letter from M. Piorry, in which that physician claims having produced the same idea more than six years ago in his lectures, and in the "Bulletin Clinique." In your article on the subject of the "New Treatment of Fractures," in THE LANCET of June 30, you have given nearly at length the most important proofs; but have omitted to furnish fully the corroborative ones, as to time, circumstances, &c. It appears to me that if it be worth the pains to establish a claim in favour of a particular practice, it is absolutely necessary to bring forward the collateral proofs. Facts in surgery are chiefly demonstrative; in medicine, speculation and hypothesis must be more predominant; a surgical fact, then, is more valuable than a medical speculation. If M. Piorry, anxious to claim the authorship of a medical theory of six years' existence, takes an opportunity of urging his priority to the origination of a doubtful hypothesis, difficult of execution, improbable and uncertain in its effect, it is but fair that the honour of the application of a practical means of undoubted utility should belong to the country in which it was first promulgated.

The 7th edition of Cheselden's "Anatomy" was published in the year 1750. Cheselden was a member of the Royal Academy of Surgery at Paris, contemporary with Le Dran, who, at that period, was the senior surgeon of the Hospital of La Charité, and to which hospital M. Velpeau has been recently attached. In an advertisement, bound up with the 7th edition of his "Anatomy," of the above date (1750), he (Cheselden) says,—"Since the

last edition of this book, I have published some observations and cases in surgery, with prints of operations, and a set of chirurgical instruments. These are annexed to a 'Translation of Le Dran's Operations,' by M. Gataker; and as some of them relate to my 'Anatomy,' I thought it proper to take notice of them here; at the same time, in justice to the merit of M. Le Dran, I would recommend a careful perusal of his book to all practitioners in surgery."* In this work, entitled the "Operations in Surgery of M. Le Dran, Senior Surgeon of the Hospital of La Charité, Consultant Surgeon to the Army, Member of the Academy of Surgery at Paris, and Fellow of the Royal Society of London, translated by M. Gataker, Surgeon, with Remarks, Plates of the Operations, and a Set of Instruments, by M. Cheselden, Surgeon of the Royal Hospital at Chelsea, and Member of the Academy of Surgery at Paris," we find, at p. 452, as follows:—"Plate 8 represents a limb distorted from the birth. B, in the plate, the bandage to cure this distortion, which consists of several pieces of linen rag dipped in a mixture of white of eggs and flour; the first point to be gained, in this case, is to get the foot out straight with the leg, holding it in that posture till the bandage grows stiff; after this, by succeeding bandages, the foot may be brought into its proper situation; there is no bandage equal to this for fractured leg; I always use it, leaving that part upon *the tibia very thin*, that if it grows loose by the abatement of a swelling, I can cut out a piece and bind it closer. Upon a journey I once set the cubital bones of a gentleman's arm that was broke, and making use of this bandage, he, the two next days, rode long journeys without any inconvenience, and at the end of forty days took it off, and was perfectly well. For further directions relating to the use of this bandage see Cheselden's 'Anatomy,' page 7. 8vo. London, 1750."

Viewing for an instant the scientific communication existing between Cheselden and Le Dran, the former Member of the Royal Academy of Surgeons, at Paris, the latter Fellow of the Royal Society of London,—considering that Cheselden's "Observations," &c., *attached to Le Dran's work*, containing, amongst much original matter, the extract I have above given,—it is neither possible nor probable that this practice could have been known through any other medium to those who now so fiercely urge their claims as its originators; and the presumption is, that MM. Velpeau and Seutin would not have attempted to appropriate to themselves the honour of the first employment of this mode of treatment in fractures, or would have been allowed to have done so by the surgical contemporaries of their respective nations, if that plan of treatment had been previously discovered and publicly announced by any other *French* or Belgian surgeon.* The British surgeon, acquainted with the sources whence I have drawn my proofs, and considering for a moment that honour is reflective, must have been repeatedly annoyed by seeing extracts in the medical and newspaper press of this country, in which high encomiums are passed on M. Velpeau, as the originator of a new plan of practice, dignified by the title of the "Appareil Immobile," and headed "M. Velpean's New Method of treating Fractures." Thus, in a leading scientific and literary journal for Oct. 26, 1837, I find the following remarks:—"At a recent sitting of the French Institute, M. Velpeau read a paper on this subject, and described *his mode, differing, as it does, from the others used by surgeons;* one plan consists in waiting for disgorgement of the member before applying the apparatus, and another is on the supposition that a moderate pressure hastens the disgorgement, and renders the inflammation abortive; he thinks all fractures ought to be reduced immediately; he surrounds the member, from the origin of the toes to the superior extremities, with a bandage, exerting a small pressure, to keep the fragments in a suitable position, and, instead of applying splints and pads, he stiffens the wrapper formed by the bandage. He at first thought of making use for this purpose of the solidifying liquid of M. Larrey, albumen, but he found it more advantageous to employ starch, as usually prepared by laundresses, in the manner of M. Seutin, of

* Gataker's preface to his translation of this work runs thus:—"The practice of surgery in England has, of late years, received such great improvements that to offer the public the sentiments of a foreign writer upon this subject may seem, perhaps, at present unnecessary. This consideration at least would, in general, have suppressed any such intention in me, but the great commendations which have been given to the following treatise in the original, will, I imagine, recommend a translation of it. In the execution of this undertaking I have endeavoured chiefly to be exact and faithful to the original, not only in justice to the author, but in respect to any connection which particular parts might have with Mr. Cheselden's notes. I shall trouble the reader no further than to acquaint him that the references which M. Le Dran has made to his (Cheselden's) book of 'Observations,' I have applied to the translation of that work.'"

* Since these remarks were written I have consulted Le Dran's work, entitled "Consultations sur la plupart des Maladies, qui sont du Ressort de la Chirurgie," 8vo., Paris, 1765; we shall have occasion to allude to this work subsequently.

Brussels, and to make use of pads of two pieces of pasteboard. The dessication of the whole apparatus takes place in from two to four days; 'this being once effected,' observes M. Velpeau, 'the member and the bandage are so exactly adapted to each other, that displacement becomes impossible.'"

M. Velpeau next remarks upon the advantages of the above method of treatment,—M. Velpeau's employment of the "*solidifying liquid*" of M. Larrey, or, in other words, the "resolvant liquid," of the same surgeon, better still, the "resolvant liquid" of M. Le Dran, who in his work entitled "Consultations sur la plupart des Maladies qui sont le Ressort de la Chirurgie," 8vo., Paris, 1765, mentions repeatedly that he would advise that this "resolvant liquid" should be employed in fractures, real or supposed cases of which he has given in his work. It is singular that this edition, published in 1765, contains not a single expression in its index or table of contents that would, in the smallest degree, lead one to infer that such treatment was advised in his work; the recommendations to employ this method of treatment are mixed up with the cases of fracture there given, *and there is not the slightest acknowledgment made, or account given, of Cheselden's strong approval of this mode of treatment.** From the year 1749, when Cheselden's "Observations" were published, to the year 1765, a period of sixteen years had elapsed.

Cheselden, in his "Observations," speaking of the employment of this mode says, "*There is no bandage so equal as this for fractured leg; I always use it.*" That it was equally valuable for fractured arm is proved by the extract above given: that he was as well cognisant of its value as those who, three-quarters of a century after his time, have claimed the honour of its discovery or employment, his published words prove. It is possible, though somewhat improbable, that this mode of treatment may not have been known, through this or any other medium, to those who now claim it as their own. The French and Belgian surgeons, in their dispute, have either forgotten or been unacquainted with the date of its origin in this country. A wise man has said, "there is nothing new under the sun;" certainly many new notions would be found, upon inquiry, "second-hand." Literary and scientific inquiries involve time and trouble, and it is too frequently inconvenient to make the effort.

The new nomenclature,—"solidifying liquid," "resolvant liquid," "albumen;" "starch, as usually prepared by laundresses, in the manner of M. Seutin, of Brussels;" and the various other dignified names for the same thing, prove no more than that the "solidifying liquid" of M. Larrey, "albumen," is the white of eggs of Cheselden; and that *starch, as usually prepared by laundresses*, is the constituent part of "flour." This play upon words is too absurd to comment upon. Since these remarks were penned, I find a writer in "La Gazette Medicale de Paris" claims for Belloste the honour of the first employment of this means of treatment. I shall take an early opportunity of proving that the credit is yet native here, and we to the "manner born," and that many of the *new customs* would be "better honoured in the breach than i' the observance." I remain, Sir, your obedient servant,

G. A. Walker.

101, Drury-lane.

* The "British and Foreign Medical Review" for October, 1838, has a long article on this subject. The editors have evidently a greater wish to advance the honour of foreign surgery than to do justice to our own. Possibly *the superadded French ingredients* may have had something to do with their favouritism,—"Le *blanc* et le *jaune* d'œuf avec un peu d'huile rosat et un *petit quantite de bon vinaigre*,"—"may be astringent, anodin, et resolutif." I would on my own responsibility affirm, that "un peu d'huile rosat" would be a much better addition to the toilet than to a surgical application; the "anodyne," "astringent," and "resolutive" properties belonging to one or both of the latter of these ingredients have not been, as yet, fully admitted by British surgeons, but they are considered by many gourmands indispensable requisites in a salad

DEAFNESS.

We have been requested to intimate, that a Committee having been appointed at the Newcastle meeting of the "British Association," held in the month of August last, for the purpose of considering and reporting on the instruments which may be deemed best adapted for assisting the hearing, in cases of deafness, the Committee will be happy in being favoured with the co-operation of such persons as may be disposed to assist their inquiries, either by suggestions, or by the loan of instruments, or apparatus, in explanation of their views. Letters, or parcels, may be transmitted, postage, or carriage paid, to the care of Messrs. Taylor, Red Lion-court, Fleet-street, printers to the Association.

THE LANCET.

London, Saturday, January 5, 1839.

In the last Number of this Journal we laid before our readers a variety of statistical information on the progress of small-pox, and on the increased frequency of that disease after vaccination. We purposely avoided making some observations on the statistical data then collected, because we were unwilling to draw any conclusions until a full view of this important subject had been placed before the public and the profession. We now proceed to communicate the further results of our researches, with the simple remark that we believe this to be the first time that the question has been examined, in this country at least, by a reference to the statistical records of the different nations of Europe. At the close of our last article we proposed to inquire "how far the protective influence of vacci-"nation was modified by the period of time "which may have elapsed since the intro-"duction of the vaccine virus into the sys-"tem of the individual." Faithful to the principle which we have already laid down, we disregard altogether individual opinions and seek a solution of our question from statistical observations, made on an extensive scale. The conclusion at which we have been compelled to arrive, from a careful study of such observations, is, "that the "susceptibility to small-pox in vaccinated "persons increases with the increase of time "since vaccination;" or, in other words, "that the protective influence of the vaccine "virus is gradually enfeebled by time."

The statistical data from which the above conclusion has been drawn are so numerous and varied, and are selected with such impartiality, from different persons, and at distant periods, that it will require a very overwhelming weight of evidence to alter the conviction which we have just expressed. But to *the proof.*

Mr. Brown was one of the first writers who distinctly announced his belief in the diminished influence of vaccination after a certain lapse of time. His work was published in 1809. The result of his investigations amongst a large number of families was, "that children who had been vaccinated "for a considerable length of time were "more readily affected by the contagion of "small-pox; that those recently vaccinated "resisted the contagion; and, finally, that "the gravity of the disease was proportion-"ate to the time which had elapsed between "vaccination and the attack of small-pox." Mr. Brown illustrated his opinion by a table of 37 vaccinated children who were subsequently affected by small-pox. Of these we find that 8 were under four years of age, and 29 above that period.

Dr. Gregory, physician to the Small-Pox Hospital, published, in 1823, a table of the relative ages of 137 persons who had been attacked by modified small-pox, *i. e.*, small-pox after vaccination. This is an instructive document in many respects.

It is unnecessary for us to reproduce it here: an analysis will suffice. Below ten years of age, we find that 5 persons only were affected; from ten to twenty, there were 76, from twenty to twenty-seven, there were 56. It is remarkable that Dr. Gregory should have attributed this striking difference to a difference in the modes of vaccinating practised at the time, as evinced in the nature of the vaccine cicatrix. Dr. Gregory, however, has abandoned, since then, his theory respecting the quality of the cicatrix, which he no longer regards as a test of vaccination; while Dr. Copland, from a consideration of the above table, expressed his opinion, so far back as 1823, that the protective influence of the vaccine virus gradually disappeared with the lapse of time.

In his "History of the Norwich epidemic," Mr. Cross had but few opportunities of observing modified small-pox; but the importance of the subject had not escaped his notice. He made numerous inquiries, among

the surgeons practising in the neighbourhood, relative to the time at which the disease occurred after vaccination. One practitioner had observed a case three years after the introduction of the vaccine virus into the system; from the answers of eight other surgeons, it appeared that the lapse of time between vaccination and the occurrence of small-pox varied from five to twenty years. None had seen a case of the disease nearer to the period of vaccination than *five* years. Mr. Cross also refers to the epidemic at Milhau, in which no cases of modified small-pox occurred within five years of the period of vaccination; all, and they were numerous, exceeded that period. Finally, we may cite Mr. Bell, of New York, who remarked, during an epidemic of small-pox, that more than *three-fourths* of the vaccinated who were attacked by that disease were between ten and twenty years of age.

In the year 1825, a committee of physicians was appointed to investigate the circumstances connected with the occurrence of small-pox after vaccination in London. From a table drawn up by the committee we obtain the following results:—

From 1 to 13 years of age....	2 cases
13 to 20.....	7 —
20 to 30.....	5 —
30 to 35.....	1 —
In adults (age not mentioned)..	2 —

The evidence furnished by the records of Denmark and Sweden is much more decisive of the point in question, as the statistical facts which they disclose are on a much larger scale than those recorded by Dr. Gregory. They lead us, with a force of conviction which it would be difficult to resist, to precisely similar conclusions.

In the epidemic of small-pox at Copenhagen, observed by Möhl, from the 22nd January, 1824, to 28th February, 1825, 412 patients were received into the hospital. Of these, 315 had been previously vaccinated; the age is noted in 267 patients, viz.,—

Below 7 years of age	24
From 7 to 11.....	42
From 12 to 23.....	191

From another table drawn up by Möhl, which we regret that we cannot give entire, we find that in 647 cases of modified small-pox (after vaccination) there occurred, up to the age of 10, only 82; from 10 to 20, 356; from 20 to 30, there occurred 209; and beyond the age of 30, only 6 cases.

Finally, by including the three epidemics which the last-mentioned writer has described, we obtain,—

From 1 to 5 years.....	152
5 to 25	1052
25 to 30	31

cases of modified small-pox.

Such evidence would seem to be almost conclusive, but in so important a subject as the one now under consideration, we must tell not only "the truth," but "the whole truth." In the Swedish epidemic of 1824, there died 560 persons, of whom 103 had been vaccinated. The whole of the latter were *above* 15 years of age, and had been vaccinated on the first introduction of the vaccine virus into Sweden.

In 1831, a fresh epidemic broke out in Copenhagen. From July 1831 to the end of 1832 were received into the hospital 574 patients; and, from the facts observed during that period, it appears "that the small-pox "attacked, with violence, such adults as "had been vaccinated in infancy; that the "disease was mild in proportion as the lapse "of time from vaccination was short; and "that adults recently vaccinated, and infants, "were exempt from the disease."

A similar modification of small-pox, according to age, is indicated in the "History of the Württemburg Epidemic," to which allusion was made in our last article; the greatest number of examples of *true* small-pox in vaccinated persons occurred between the ages of fourteen and twenty-seven. Not a single case presented itself below the age of fourteen. Does not this also lead strongly to the idea, that the vaccine virus afforded a protection during the first few years of life, which it failed to do after a more considerable lapse of time?

Dr. Heim has given an elaborate table of the vaccinated who had been attacked by *modified* small-pox during the Württemburg epidemic. The total number of attacks after

vaccination amounted to 1055. The average annual number of cases during the first two years after vaccination was 11; in the next twelve, it amounted to 48; in the next five to 41; and in the next six to 15; or, by arranging the table into periods of ten years, we obtain,—

Below 10 years of age	94
From 10 to 20	407
From 20 to 30	502
From 30 to 35	22

The coincidence between this table and those which we have extracted from the records of Denmark, Sweden, and England, is most striking. Some uniform cause must be in operation to produce such analogous results in different kingdoms, and during different epidemics of small-pox. The extensive numbers which furnish the basis of the above tables speak more strongly than any argument that we could employ. The reader need only run the eye along their columns to perceive, at a glance, how powerful is the protective influence of vaccination at an early period of life; and how remarkably that influence diminishes in proportion as we recede from the date of vaccination; for nearly the whole of the individuals to whom the tables refer were vaccinated, as is the custom, in infancy. But it may be urged, perhaps, that this gradual increase in the number of patients, according to age, is an ordinary law of the propagation of small-pox. This objection can be readily answered. Mr. Cross has given a table of the ages of 530 unvaccinated persons who were cut off by small-pox during the Norwich epidemic. From the number of deaths we should calculate the number of attacks at different ages, in order to obtain perfect accuracy, and render this table fit to be compared with those which we have already produced. With this allowance the Norwich table presents a striking contrast to those of Sweden, Denmark, and Germany, and demonstrates that progressive increase of the numbers attacked is not a law of natural small-pox.

In Mr. Cross's table we find,—

Under 2 years of age	260
From 2 to 4	132
4 — 6	85
6 — 8	26
8 — 10	17
10 — 15	5
15 — 20	2
20 — 30	2
30 — 40	1

Thus, according to the account of Mr. Cross, there were attacked by natural small-pox (in unvaccinated persons),—

Under 10 years of age	520
From 10 to 20	7
20 — 30	2
30 — 40	1

Mathieu has furnished us with a table of the ages of persons affected with natural small-pox in Paris, during the year 1830. This table is a more useful one than the former, because it enables us to compare cases with cases, instead of cases with deaths:—

Up to 10 years	203
10 to 20	63
30 — 40	55
40 — 50	2

Compare this last table (which is probably a fair expression of the relative frequency of natural small-pox according to age) with the tables of Gregory, Möhl, or Heim, and what is the result? A conviction that *natural* small-pox occurs with great frequency up to twenty years of age, and diminishes after the age of forty; while, on the contrary, *modified* small-pox is rare *before* the age of eight or ten years; becomes gradually more frequent as the individual's age recedes from the period of vaccination, and again becomes rare at the time of life when the susceptibility of small-pox seems to be extinguished altogether.

Heim mentions the ages of 37 persons who were attacked by natural small-pox during the Württemburg epidemic; of these were,—

Under 3 years of age	20
From 15 to 22	7
30 — 40	4

Of 5 no knowledge as to age was obtained.

This table leads to a similar conclusion as the Parisian one of M. Mathieu.

We have still to notice several statistical facts, which, though of less moment than the preceding, as being on a more confined scale, afford strong confirmation of the progressive decline of the vaccine influence. Thus, to take the following from the "Bibliotheque Universelle" of Geneva:—

2 months after vaccination	1
2 to 6 months	2
6 — 12	4
1 — 5 years	34
10 — 15	36
15 — 20	9

Or a still more striking example from the Milan Hospital, cited in the "Annali Universali," of February, 1826, which shows that of 72 vaccinated persons affected with small-pox, there were,—

Under 2 years of age	1
5	1
7	1
11	5
From 12 to 18.....	35
18 — 24	29

During the epidemic at Hambourg in 1823, forty-three cases of small-pox were noted, amongst which twenty-six occurred in individuals who had been previously vaccinated. The relative ages were,—

Up to 1 year	1
From 1 to 10	2
10 — 15	5
15 — 20	8
20 — 30	10

The above table looks as if it were constructed with the object of proving the progressive decline of vaccine influence according to age, so regularly does the proportion of cases increase up to the age of thirty.

The accounts which have been given by the different district physicians of Württemburg fully confirm the general result announced by Dr. Heim, and mentioned in another part of this article. We shall select one or two of them by way of example.

In one epidemic, observed by Dr. Rösh, 58 persons were affected with small-pox after vaccination, of whom there were,—

Under 10 years of age.....	7
From 10 to 20	29
20 to 30	22

Of the children *under* ten years, five were attacked in a very slight manner, while the disease proved most fatal amongst the 22 individuals between 20 and 30 years of age. Here, then, was not only the quantity, but the quality, of the small-pox virulence affected by the lapse of time since vaccination.

The number of cases observed by Dr. Fritz amounted to 216; the respective ages were,—

Under 10 years	20
From 10 to 20	59
20 to 30	137

The epidemic at Ramningen lasted twelve weeks, and attacked 37 individuals, of whom 32 had been previously vaccinated. Of the latter there were,—

Under 10 years.....	0
From 10 to 20	7
20 to 30	24
At 33 years of age.....	1

This is a striking example. Up to ten years of age, vaccination had protected the whole community; one-fourth of the cases occurred between the ages of ten and twenty; past this latter age we find three-fourths of the entire number of cases; the vaccine influence had now nearly disappeared. Comments upon such facts are almost superfluous. It were easy for us to multiply statistical citations in support of the proposition laid down at the commencement of the present article; but if we have failed to convince any of our readers, there can be no advantage in continuing to "heap Pelion upon Ossa." It is, however, our duty to mention opposing facts. We are acquainted with only two statistical documents which seem to contradict those published by Gregory, Heim, Möhl, and Wendt. The first is the table of Mr. Gibson, which is appended to Mr. Thomson's letter to Sir James M'Grigor; but the value of this table is very considerably diminished by the circumstance that its author seems to have included cases of varicella, as well as of small-pox.

M. Dufresne's table, like that of Mr. Gibson, is a resumé of cases observed in private practice. The results certainly differ, in a very material degree, from those

at which we have already arrived. It is as follows:—

Up to 2 months	1
From 2 to 6	2
6 to 12	4
From 1 to 5 years	34
5 to 10	36
10 to 15	20
15 to 20	9

Here the number of cases occurring between the ages of one and ten years is much greater than could have been expected after an examination of the extensive tables furnished by MÖHL and HEIM. We cannot explain this difference; but must, at the same time, remark, that DUFRESNE's table forms the exception; those of MÖHL, HEIM, GREGORY, and WENDT, the general rule. The evidence on the one side is overwhelming; on the other, it yet remains to be adduced, for we have searched for it in vain.

In our next article we propose to show, "How the gravity and danger of modified "small-pox is generally proportionate to the "lapse of time since vaccination; how revac- "cination fails in children, but succeeds "in adults; and, finally, how far the ex- "pediency or inexpediency of revaccination "may be determined in the present state of "our knowledge."

WE are asked not less, probably, than a hundred times in every year, whether it be necessary that a candidate for the license of the Apothecaries' Company should serve an apprenticeship before he is entitled to be examined by that body for authority to practise as an apothecary in England and Wales. The best answer that we can give to the questions which are thus so repeatedly asked, will be found in the 15th section of the Apothecaries' Act, which we here reprint, *verbatim*:—

"15. Provided always, and be it enacted, that no person shall be admitted to any such examination for a certificate to practise as an apothecary, unless he shall have served an apprenticeship of not less than five years to an apothecary, and unless he shall produce testimonials to the satisfaction of the said Court of Examiners, of a sufficient medical education, and of a good moral conduct."

This is clear enough. But it may be said that the Worshipful Hags of the Hall have become *liberal*, and that they do not scrutinise with nicety the testimonials which are presented by candidates, upon which documents it may appear, *prima facie*, that the apprenticeship of five years has actually been served. In other words, it may often happen that candidates producing such testimonials have not served an apprenticeship of five years, and yet succeed in obtaining the license of the Company. Of what value, however, is a document so obtained? NONE. It is not worth the paper on which the license is written.

Is it true that many licenses of this description have been granted by the Company? If private report speak correctly, there exist not only dozens, but hundreds, of such worthless certificates. In what situations are the possessors? First, if they have obtained their license by fraud, they are liable to indictment and imprisonment. Secondly, if they have been merely careless, and have obtained the license without complying with the terms of the 15th section of the Act, and served a *bona fide* apprenticeship of five years duration, *then* are they liable to be prosecuted for practising as apothecaries, precisely as are other unqualified medical practitioners. Further, if there stand on their books debts to the amount of two thousand pounds, not one of those bills could be recovered in a court of law, if the debtor sued were able to prove, and did prove, before a judge and jury, that the plaintiff had obtained his license from the Company without having previously served an apprenticeship of five years to an apothecary. The Company may be generous,—may be indifferent about the apprenticeship; but the Hags, with all their "liberality" and conjuration, *cannot alter* THE LAW. The 15th section of the Act is imperative, and every person who may dare to violate its conditions, must take the consequences. The Hags would be the last to afford him protection, if it were in their power to furnish it; but these very parties—the Worshipful

Company — on a prosecution being instituted, would find that they had themselves become culprits by having admitted candidates to an examination for the license, and having granted licenses in cases where the fact was established that a previous apprenticeship had not been served.

The answer which we have thus given will, we know, be found to be correct, although we much fear that it will not prove satisfactory to all our interrogators on the subject.

UNIVERSITY COLLEGE AND HOSPITAL.

EXPULSION OF ELIZABETH OKEY.—RESIGNATION OF DR. ELLIOTSON.—MEETING OF STUDENTS.

On the morning of Friday, Dec. 28, Dr. Elliotson sent in his resignation as lecturer on medicine at University College, and as senior physician to the North London Hospital. On the evening of the same day he entertained a numerous body of the students of those institutions at a dinner party in Conduit-street, when he addressed them very energetically on the step which he had taken. The cause of the resignation may be thus briefly stated:—

Early in June last the Medical Committee of the hospital held a meeting to take into consideration some "published statements" respecting animal magnetism, which had appeared in THE LANCET. Dr. Elliotson did not attend this meeting, at which resolutions were carried, requesting the Doctor to refrain from further "public exhibitions" of mesmerism, at the same time stating that they did not wish to interfere with its employment as a remedial agent when he chose to employ it. In answer to these resolutions the Doctor said that "*no* consideration" should prevent his pursuing the investigation of animal magnetism; that he had never made a public exhibition of it, but had only employed it remedially; and, as it was a subject in which many new facts were likely to be developed, had simply given clinical lectures and demonstrations upon it to the pupils, when a great number of scientific and eminent men had attended, but only on special invitation, after their urgent requests to be present. However, he would refrain from further exhibitions of them in the theatre, though he should forward to the Committee the names of such gentlemen as might in future apply for permission to witness the experiments, leaving it to the Committee to sanction the admission to the exhibition in the theatre of such persons as they chose to approve of. In a short time a list of applicants was sent to the Committee, who did not read it, but merely replied, that they could not sanction *any* exhibition that was so entirely foreign to the objects of the hospital. In answer to this Dr. Elliotson stated his full conviction of the reality of mesmerism, and his belief that the light which his experiments would throw over the operations of Nature would equal, if not exceed, that elicited by all other discoveries, and that mesmerism was of great physiological and therapeutical importance. Here the correspondence dropped, but mesmerism was practised in the hospital with undiminished frequency. No *public* exhibition in the theatre took place, it is true, but numerous small parties were entertained in corners of the wards with the effects of mesmerised water, gold, and other substances on the notorious Okeys, and the clinical clerks were employed in the wards for an hour or two daily in manipulations upon epileptic and other patients.

The public are aware of the thorough, complete, and unequivocal exposure of the humbug of mesmerism, which was made in Bedford-square, in August last. Dr. Elliotson left England at the end of that month, and was absent for four or five weeks, but the patients were mesmerised as usual, in compliance with his directions, during that time the girls Okey being on a visit at Dover. In October the House-Committee of the hospital held a meeting to consider the case of Elizabeth Okey, who had now, excepting for about a month, been in the hospital ever since April, 1837. The Committee were now informed that Elizabeth Okey had been readmitted for ischuria, a fresh complaint, totally unconnected with her former malady.

At a subsequent meeting of the House-Committee, held early in December, Dr. Elliotson attended, and gave it as his opinion that it was still necessary to retain Okey in the hospital, as she was too ill to be discharged; and, in answer to questions from the Committee, he stated that Okey was now entirely free from attacks of delirium, excepting when brought on by mesmerism. In reply to a remonstrance from one of the members, that he was in the habit of "taking away this poor girl's senses occasionally," the Doctor returned no answer. Dr. Elliotson also stated, on this occasion, that having heard the nurse of the ward say that Okey had occasionally passed opinions as to the issue of certain cases then under treatment, he had taken her into the men's ward, in the twilight, where she had prophesied respecting the termination of some of the diseases, and these prophecies were written down and given, in a sealed paper, to the apothecary. He had not, however, taken her into the men's ward without first inquiring of the nurse if that

step might be taken with propriety. In addition to the evidence given by the Doctor himself on this subject, the nurse gave an account of the manner in which the girl conducted herself when attended by Dr. Elliotson on those occasions. She said, that on approaching the bed of a certain patient, Okey gave a convulsive shudder, and when asked the reason she replied that "Great Jackey was on the bed," meaning, according to her own subsequent explanation, that "Great Jackey" was the "angel of death!" She shuddered slightly only at the bedside of another patient, "because *Little* Jacky was seated there!" These prophecies were accredited by the Doctor, as correct indications of the fate of the patients respecting whom they were expressed. The effect of these proceedings, repeated in the same ward (death on one occasion having followed the visit of the girl), was of such a kind on the patients, that, to use the words of a gentleman who had witnessed them, "the ward was in a complete flurry."

A few days since the Council of the College, additionally instigated by these proceedings, requested the Committee of the Hospital to take steps for the immediate discharge of Okey, and the prevention of further mesmerism in the wards. Dr. Elliotson did not at first receive the former part of this request, but was "respectfully requested to discharge Okey;" but on the request respecting the further pursuit of mesmerism being communicated to him, he immediately sent in his resignation to the Council.

We have stated that on Friday, Dec. the 28th, a very large number of the students were invited to dine with Dr. Elliotson. On Wednesday, Jan. 2nd, a meeting of the students was held in the Anatomical Theatre for the purpose of hearing read the letter of resignation, copies of which had been freely circulated amongst them. This meeting was got up suddenly by the friends of Dr. Elliotson, and, after the reading of the letter, was adjourned to Monday, Jan. 7th, at one o'clock. The Doctor had stated in the letter that his reason for resigning arose out of the undue interference of the House-Committee and the Council with a patient who was under his care in the hospital. He also requested in it that the fees which had been paid by students for the second half-session of his lectures, should be returned to his class, and added, that he would never again enter either the Hospital or the College. Here the matter at present rests.

In conversations which we have had with a great number of medical men, and of friends of the University, we find that the conduct of the House-Committee and the Council in this transaction, has received a very hearty approval. We do not find that the integrity of Dr. Elliotson has been at all impugned in this affair, but it is considered by many of the best informed members of the profession, and by the best friends of the University, that the Doctor's secession from the establishment will be attended with immense advantage both to the College and the Hospital, and that his continuance in the two offices,—considering the opinions which he honestly holds on the subject of mesmerism,—would be attended with great injury to the utility of the Hospital, and the certain, the inevitable, ruin of the medical department of the College. It is generally thought that the Council is relieved from taking a harsh step with regard to the Professor, by his sudden and prudent withdrawal from the establishment. The professors of the College, as well as the most considerate men in the profession, are at length convinced that such a humbug as *mesmerism*, and the *science of medicine*, cannot exist co-ordinately in this country, and that of all the various species of quackery which were ever invented, there is not another that is either so dangerous or so ridiculous as that which has been impudently styled "Animal Magnetism." When Dr. Elliotson became a *mesmerist*, he ceased to be a "physician." The escape of the medical department of the University from complete ruin, may be attributed to the temperate but firm conduct of the House-Committee of the Hospital, and the Council of the College.

ON

LUNATIC ASYLUMS AND ESTABLISHMENTS FOR INVALIDS

IN THE

North of Germany and in St. Petersburg.

BY M. LEURET.

(Concluded from page 448.)

THE lunatic asylum of St. Petersburg is situate about eight miles from the city, and consists of a handsome building, which is composed of a central portion, enclosed by two wings. In the centre part are placed the apartments of the medical officers and servants, two chapels and offices; the wings are occupied by the male and female patients. Both wings are similarly arranged; a long corridor runs from one extremity to the other, with chambers opening on each side. Each chamber contains two well furnished iron beds; the windows are large, and placed at five feet above the level of the ground; the upper part of each window opens on a revolving hinge. The ground and third floors are arranged in a similar manner. To each division are attached a dressing-room, a saloon, and water-closets of the best construction. The violent patients, and those likely to injure themselves, are placed on the ground-floor,

in rooms which are carefully lined throughout with a soft, yielding material. During the night a chaise percée is placed in each room, and the whole length of the corridor is warmed by numerous stoves. Maniacal, or furious patients, are restrained by means of straight-waistcoats, which are laced behind, and by large leather gloves, in which the hand and a part of the forearm are confined. The baths are not well arranged, but there are several vapour-baths, a form to which the Russians are extremely favourable. The food is excellent, and everything connected with the table is neat and clean. The meat is cut up in the kitchen, in order to dispense with the use of knives; a silver spoon and fork, however, are placed beside each patient. When completely filled, the hospital is capable of containing 130 persons. They are waited upon by 120 servants, in addition to whom are 20 keepers, three physicians, one inspecting physician, four assistant-surgeons, one apothecary, and two assistant-apothecaries.

The annual cost of each patient, every expense included, is 750 roubles, or about 31*l.* 5*s.* The physician, M. Ruhl, is most careful in avoiding the employment of any means calculated to terrify the patients; he likewise endeavours to distract their minds by light and agreeable occupations. He makes those, who already know how, write certain pieces, which are dictated to them; those who do not know how to write receive instruction. Some of the patients are employed in making trifling objects in pasteboard; the women knit or sew, or smooth the linen with wooden cylinders. Paralytic and epileptic patients are not received into the hospital; the only persons admitted are such as are curable, or presumed to be so.

SACHSENBERG.

On my return from Russia I visited successively the different lunatic asylums at Sachsenberg, Berlin, Pirna, Halle, and Sigberg.*

The disposition of the hospital at Sachsenberg is extremely faulty; the most violent, furious lunatics are placed beside those who are most tranquil, and constantly annoy the latter by their cries or imprecations. The beds are fixed to the wall, and thus expose the patients to the danger of contracting rheumatic affections, and to other obvious inconveniences. The water-closets are also fixed in the walls, an arrangement which has invariably been found to be productive of uncleanliness and its consequences. The chambers contain from two to seven or eight beds each; the pauper patients occupy the ground floor, and above them are placed such patients as can afford to pay for superior accommodation. The number of patients at the time of my visit amounted to 150, although the hospital is capable of containing 200; since its foundation in 1830, the numbers admitted amounted to 335, of whom 183 were males, 152 females. During this period the deaths were 41 males, 20 females, and the cures 52 males, 47 females.

* We have considerably abridged this portion of M. Leuret's Memoir.

BERLIN.

In Berlin the lunatic, syphilitic, and itch patients are all collected together in a building attached to the Hospital of La Charité. The different classes of patients are placed on different floors, which have no communication with each other. The various classes of lunatics are also mixed pêle-mêle together, and the whole arrangement evinces not only neglect, but sordid economy on the part of the Government.

HALLE.

The lunatic asylum of Halle is an old prison, which, from the want of another building, has been appropriated to the reception of the insane. Materially speaking, it is, perhaps, one of the worst lunatic asylums in existence, but much has been effected to counteract unavoidable evils by the assiduity and talents of its director, M. Dameron. The number of patients is about fifty, and of these I only saw four who were not occupied. The patients make cravats, stockings, nets, mats, &c., and the price of their labour is partially divided amongst them. The effect of thus occupying lunatics is most beneficial.

One of the greatest faults which I had to find with most of the establishments visited by me during my late voyage, was their extent—the large number of patients received into each asylum. Such a circumstance renders the moral treatment of mental alienation totally impossible. To reform the disjointed imagination of the lunatic, to eradicate the evil passions which are generated by his disease, it requires the constant and unwearying attention of the medical man. Such attention can only be bestowed upon a few patients at the same time. To the medical ideas which generally prevail respecting madness, we are to attribute, perhaps, the error of crowding several hundred patients into the same building. Too many physicians regard madness as invariably depending on some cerebral lesion, and to this lesion they consequently address their whole treatment. Thus the common mode of treatment, even in cases of monomania, consists almost exclusively in the use of physical means. One patient imagines himself to be a king; another, emperor; a third the Pope; a fourth the Devil, &c.; the same treatment is applied to all. A bath, leeches to the anus, purgatives, and an issue. I am acquainted with one asylum, of high reputation, containing four or five hundred beds, in which, during a period of ten

years, each patient on his admission into the hospital, was treated with an application of twelve leeches to the anus. Thus, pharmaceutical preparations and coercive means are in high repute, while the moral treatment of insanity is almost totally neglected. In the present state of things a large number of lunatics may be physicked by a single physician, but scientific treatment imperatively demands small hospitals and a limited number of patients. Under one system we meet nothing but ill success; the latter would reward us with numerous cures.—*An. d'Hyg. Publique*, Oct. 1838.

REPORT

OF THE

MEDICAL AND SURGICAL SOCIETY

OF

NEWCASTLE-UPON-TYNE,*

On the Present State of the Medical Profession.

The Society is desirous to submit to the notice of the profession generally, the following statement of some of the more prominent evils, which have, for a length of time, weighed heavily on the interests both of the public and of individuals, with a brief outline of the measures calculated, in the opinion of its members, to correct the abuses complained of. As, however, their effectual reformation can be obtained only by the vigorous and united exertions of the whole profession, and not by detached and unconnected efforts, the Society is anxious to invite the co-operation of professional gentlemen in other places, and to recommend to their immediate attention the important questions comprised in this Report. The feeling of the profession at large, as to the defects most urgently requiring amendment, and as to the general principles on which such amendment should be founded, would, in this way, become apparent; and with a view to the settlement of disputed points, the Society would suggest the expediency of a conference being held in London, or elsewhere, composed of deputations sent from different parts of the empire, whose labours might be directed to the arrangement of a specific plan of Medical Reform, which, if incorporated in a Bill to be introduced into Parliament, and duly supported by petitions, might be reasonably expected to meet with the consideration of Government and the Legislature.

(Signed)

T. E. Headlam, M.D., President.

T. M. Greenhow, Secretary.

Newcastle-upon-Tyne,

Nov. 27, 1838.

This Society, on surveying the condition of the medical profession, cannot but be strongly impressed with the varied and incongruous materials of which it is composed; they find its members, notwithstanding the perfect identity of their avocations, consisting of practitioners authorised by many different colleges and corporate institutions, from whom have been required unequal courses of study, and dissimilar examinations into the extent of their professional and scientific attainments. They discover, also, that these institutions have been found inadequate to the fulfilment of their intended purposes; that, consequently, persons are engaged in practice, who have not been authorised by any of them, and that a numerous class of individuals are allowed to prey upon the community, under pretence of having become acquainted with operations and modes of curing diseases, unknown to those who have endeavoured, by an appropriate education, to qualify themselves for the treatment of the various infirmities and injuries with which the human frame is liable to be afflicted.

Public safety demands that all persons engaged in the practice of medicine and surgery should be duly qualified, by a previous course of study, to discharge the important duties they are called on to perform; and this Society deems it incumbent on the Legislature to provide, that no person shall be permitted to enter upon the execution of those duties, who has not been properly tested as to his acquirements, and publicly declared to possess the requisite degree of qualification.

In consequence of the imperfection and defective administration of the laws at present in existence, they have afforded no security to the public, and have yielded no adequate protection to the legally authorised practitioner, against an unfair and unjust competition with individuals possessing neither license nor diploma, and who are unable to produce satisfactory evidence of their having devoted sufficient time or attention to the attainment of medical and surgical knowledge.

Quackery, of every description, is allowed to exert its baneful influence on society; and, while its miraculous achievements are continually paraded before the public eye, thousands of its credulous and unsuspecting victims are daily submitting themselves to the pain and torture of secret processes of treatment. It is to be deplored that any part of the public revenue should be derived from the indirect encouragement of a system re-

* The Society consists of more than fifty members.

plete with fraud, and productive of most disastrous consequences.

Chemists and druggists are in the constant habit of prescribing for diseases, with whose nature they can be but very imperfectly acquainted.

The Corporate Bodies presiding over the profession appear to be quite unequal to the correction of these abuses. The London College of Physicians have at all times acted on a system of exclusiveness and monopoly, and have never displayed the slightest anxiety for the well-being of the profession at large. The charter of the College of Surgeons confers upon that corporation no control over persons practising surgery; submission to their examinations and by-laws being entirely *voluntary*. The Company of Apothecaries have, by Act of Parliament, power to prosecute all persons practising as apothecaries without their license. They have, however, exercised this power in a very limited number of instances, nor do they possess the means of exerting it in a manner likely to counteract the magnitude of the evils in question.

Medical education requires considerable amendment. The long space of time occupied in the apprenticeship of most general practitioners is usually passed in a very unprofitable manner. Apprenticeships are objectionable in many respects, and are totally inefficient as a medium of professional improvement; hence, after their termination, a course of study requires to be entered upon, which must, of necessity, be compressed into a period by far too limited for the attainment of the object in view. The information acquired is almost inevitably of a superficial character; for, in addition to hospital practice and dissections, the student is compelled to attend several lectures daily on a variety of subjects; from all of these it is impossible he can derive full advantage. Clinical instruction is much neglected, and the student completes his prescribed curriculum with a very inadequate preparation for the practical duties awaiting him at the bed-side of the sick.

Certificates of attendance on lectures and hospital practice are frequently granted without inquiry as to the student's diligence, and in some instances are surreptitiously obtained.

The examinations for licenses and diplomas are conducted not unfrequently in a cursory and inefficient manner. They do not afford an opportunity of ascertaining the *practical* acquirements of the candidate, nor the extent to which his knowledge is the result of *clinical* observation and experience. The College of Surgeons in London require certificates of candidates having studied the various departments of medical science, but their examinations are confined to anatomy, physiology, and surgery. The Society of Apothecaries take no cognisance of surgery, therefore their licentiates being legally qualified, are at liberty to practise *every* branch of the profession without having been examined in surgery, or required to produce evidence of having been engaged in the study of that important subject.

There are in the United Kingdom of Great Britain and Ireland, not fewer than 19 corporations having power to grant degrees in medicine and surgery, and differing essentially in the extent and duration of the curricula they enjoin. The Society is of opinion that all of these might be advantageously superseded by one institution being placed at the head of the profession in each division of the empire, whose privileges should be reciprocal, and whose executive officers, elected by the members at large, should hold periodical conferences, for the purpose of establishing uniformity of operation; that such institutions should have entire control over education, and the granting of degrees and licenses to practise, together with all other matters relating to the medical profession; that the course of instruction and test of qualification should be the same in each; that, from one or other of them, all persons engaged in the practice of medicine and surgery should be required to possess a diploma or license; that, to individuals thus authorised, the law should extend a suitable protection; and that proper measures should be enforced for the suppression of unqualified practitioners.

The Society would suggest, as a means of effecting the last-named object, that every person, before commencing practice in any town or other locality, should be required to obtain a certificate from a magistrate, giving him permission to that effect, which should be granted on the production of satisfactory testimonials of qualification, and that, after having been thus authorised, his name should be duly registered. Persons presuming to practise, whose names have not been so registered, should be subjected to a penalty on summary conviction before a justice of the peace.

The rapid progress of science during the present century, in conjunction with increased facilities for the attainment of medical and surgical knowledge, have fully proved, that any attempt to constitute an arbitrary division of diseases, and to consign the treatment of them to different classes of practitioners, according as they affect the external or internal parts of the body, is not only unscientific, but impracticable; and, as the physician and the surgeon must be guided by similar principles in combating disease, whether involving the surface or the interior of the human frame, the education of all practitioners should, in the opinion of this Society, be regulated by one common standard. Those distinctions in rank which have hitherto subsisted (not, perhaps, without good effect) would thus be

rendered unnecessary, since there could be no longer any rational ground for separating into different grades men who would be identified not less in education, than in the nature and object of their pursuits. Such uniformity, if established in this as *in other countries*, would place practitioners, whether of medicine or of surgery, on an equal footing; but would not, in the least degree, prevent individuals devoting their energies to the prosecution of any particular department of professional duty, which inclination or other circumstances might lead them to adopt in preference to another.

The task of preparing the medicines prescribed by the general practitioner, devolves, almost universally, on the apprentice of the latter. That material benefit might accrue from a well devised scheme of pupilage, there can be no question: but apprenticeships, as at present conducted, have ever been productive of unhappy results; and in no respect does the unfavourable tendency of this system appear more conspicuous than when viewed as an instrument for executing the responsible duty of dispensing. The abolition of apprenticeships, so far, at least, as this department is concerned, would be highly expedient. The Society is of opinion, that a charge so important might, with greater safety, be confided to an apothecary or dispenser, who had been examined in pharmacy, and had obtained a specific license for the purpose in question. Such substitute for the apprentice would, the Society believes, be most desirable, not less for the comfort and convenience of the practitioner, than for the welfare and security of his patients.

This proposition, if acted upon, might have an additional good effect in terminating the absurd method by which at present the majority of general practitioners seek to be remunerated, viz., by a profit on the medicines they supply.

The foregoing representations suggest the desirableness of obtaining,—

1. An improved system of education.
2. A more efficient method of examination.
3. One governing body to preside over the whole profession in England, Scotland, and Ireland.
4. Uniformity of education, and of grade among practitioners.
5. Adequate protection for legally authorised practitioners.
6. The prevention of unqualified persons.
7. The suppression of quackery.
8. The separation of the practitioner and the dispenser in the same individual.
9. The abolition of apprenticeships as at present constituted.
10. The institution of licensed dispensers.

UNNECESSARY CORONER'S INQUEST, AND EXTRAORDINARY VERDICT.

To the Editor of THE LANCET.

SIR:—If you consider the accompanying account of a coroner's inquest of sufficient interest to be inserted in THE LANCET, it is at your service. Your obedient servant,

EVAN DANIEL.

43, Bishopsgate-street, Within,
Dec. 28, 1838.

I was consulted on the 18th instant by John Hutchings, living at No. 5, Sugarloaf-court, Long-alley, Eldon-street, Finsbury, in the parish of Shoreditch, aged 33 years, who had been a wine and spirit merchant's porter from his boyhood; his own statement was, that two or three weeks before, while standing on a block of wood, it slipped from under him, and he fell down and injured his back.

A few days (I believe he stated three or four days) previous to the 18th, while stooping, a lad in the same employment, to call his attention to something, touched him with his foot on the perineum, which gave him great pain. I examined the part, and found a phlegmonous swelling on one side of the rectum. Twelve leeches were applied; mild saline aperients administered.

19. The leeches and medicine repeated, and the part affected frequently fomented and poulticed.

20. Medicine and poultice continued.

21. I opened the abscess freely, and discharged several ounces of healthy pus. Until this day he was kept on low diet; he was allowed, after the abscess had been opened, to partake of some excellent beef-soup, his employer having sent to him, from his own house, about a gallon; towards evening symptoms of delirium tremens appeared; from this time he had mutton-chops and porter, ammonia, combined with opiates, alternately, with doses of gin and water; mustard poultices applied to the neck and spine; the delirium continued during the 22nd and 23rd; he died exhausted about 12 o'clock, P.M.

On the evening of the 25th, to my no little astonishment, I was informed by Mr. Finer, the out-of-door medical attendant of the parish or union of Shoreditch, that an inquest was to be held on the body the following day, at half-past five, at the Dolphin public-house, Long-alley. I explained to this gentleman that the man died of delirium tremens, and I considered an inquest quite unnecessary and uncalled for. However, Sir, an inquest was held; *I was not summoned to give evidence;* the jury were kept from their homes and business until ten o'clock at night, and, on the hearsay evi-

dence of Mr. Finer, and under the direction of Mr. Baker, the jury brought in a verdict of "Died from Natural Causes!!"

Mr. Baker has just as much right to hold an inquest on every person who may die of typhus fever, small-pox, or any other disease, as he had on poor Hutchings, who actually died of delirium tremens. I endeavoured to ascertain who gave information to Mr. Baker, but I have been unable to find out. Has he paid scouts in every district? Will the County of Middlesex pay upwards of five pounds, the expences of this inquest? Are tradesmen to be taken from their business for four hours and a half on such pretences, and have to pay for it in the form of county rates?

CAUSE OF THE DELETERIOUS ACTION OF BURNING CHARCOAL ON ANIMALS.

To the Editor of THE LANCET.

SIR:—You were so kind as to insert in THE LANCET, bearing date December 8th, 1838, an opinion of mine with regard to *the cause* of the deleterious effects which are experienced by the animal creation when subjected to the vapours produced by the combustion of ordinary charcoal. This opinion was the fair and honest result of inductive reasoning, founded upon theoretical and personally practical experience; and, although it happened to be novel, and at variance with the ideas of every scientific physiologist and chemist of my acquaintance, at the time of its publication, I am happy to state that it has been strengthened by subsequent investigations, and that scepticism has diminished, and an interest awakened, which cannot fail to set this important subject at rest.

The substance which has been already described as being a volatile product, invisible unless sought for, and to be retained only by condensation upon a comparatively cool surface, is a compound containing carbonate, hydrochlorate, and sulphate of ammonia, associated with another body of a very peculiar nature, which, in most of its properties, closely resembles *cyanogen!*

Having enlisted the services of a very able friend (Mr. Wm. Herapath, the well known professor of chemistry at the Bristol Medical School) we will conjointly give you the earliest intimation of our results, so soon as a sufficient quantity of the matter in question can be collected, and a more rigid examination instituted. The subject being embarrassed with considerable difficulties, will, of course, require both time and leisure from other pursuits,—meanwhile believe me very truly yours,

CHAS. T. COATHUPE.

Wraxall, Dec. 31, 1838.

STYPTIC QUALITY OF THE MATECO.

To the Editor of THE LANCET.

SIR:—In the course of last year a young mercantile friend of mine brought with him, from South America, a small quantity of the leaves of a plant which he stated was much used, and in high repute, as a styptic of great power and efficacy, immediately closing the extremity of a bleeding vessel in profuse hæmorrhage. At the same time he gave me the following account of its accidental discovery:—"A soldier was wounded in the field of battle, and left bleeding so profusely as to afford little or no hopes of his surviving. The occurrence of syncope so far checked the hæmorrhage as to revive him and enable him to perceive his threatened danger. He immediately exerted what little strength he possessed to fill the wound with the leaves of a plant which accidentally was within his reach, and the effect was so speedy and complete as to enable him to rise from the place where he fell, and he survived to record the means which he supposed had saved his life. My opportunities of proving the truth of this information have neither been extensive nor frequent, but they are quite sufficient to induce me to draw the attention of surgeons to a trial of the remedy; and as the subject is familiar to every well-informed person who has visited South America, there can be no difficulty in procuring some leaves for further proof of its efficacy. I do not know that I should have troubled you with this communication had I not within these few days derived further additional information on the subject, from Mr. Wm. Wheelwright, a gentleman who is now in Liverpool, who has resided in that country for 15 years, and is well known as the successful projector (now carrying into effect) of steam navigation in the Pacific, and also between Valparaiso, Panama, and England. The name of the plant from which the leaves are collected, is *Matèco*, and it is to be met with in the Province of Chequas, the eastern extremity of Bolivia, where, he says, it is not only used for suppressing the bleeding from wounds externally, but also as a diuretic, of which latter quality I have no knowledge, but I will not fail to use it at the first favourable opportunity which may offer, should I be able to procure it in a larger quantity than I at present possess. I am, Sir, your obedient servant,

THOS. JEFFREYS, M.D.

Liverpool, Dec. 24, 1838.

LITHOTRITY PRACTISED IN IRELAND ONE HUNDRED AND EIGHTY YEARS AGO.

THE following case has been recently cited by Mr. Crampton, as an example of lithotrity performed more then a century

and a half ago. It is sufficiently curious to deserve notice, even though it may be supposed that the calculus was broken up in the urethra, and not in the bladder. The original paper is lodged in her Majesty State Paper Office in Ireland:—

The State of Sir H. Sidney's Bodie, MS., Ireland, 1559, in Her Majesty's Office of Papers and Records of State.— My Lord President, being of the age of xxxvi. yeares, went into Ireland a hole man, not touched with the stone, and so remained one yeare and a half or thereabought, and then, after long grief, avoided two stones which were very big, such as few men have been known to have avoided. After this he took his journey into the north parts of Irelande, and so continued void of pain or grief until his arrival in Englande, which was about 8 weeks after, and then at Chester felt the like grief as at first, and so continued in pain until Christmas Eve; at that time being searched with surgeons he avoided one other stone, *broken by the surgeon his instruments in diver pieces*, for that it was so great that otherwise it could not be taken out, for all the pieces laid together might make the quantity of a nutmegge.—*Collin's Lives and Actions of the Sidneys*, p. 95.—*From the Dublin Journal*, Jan. 1839.

PATHOLOGICAL SOCIETY OF DUBLIN.

We are happy to announce that a society has been formed in Dublin, with the object of promoting and diffusing the knowledge of pathological anatomy. If the Society evince but one half of the industry which animates the "Anatomical Society" of Paris, it cannot fail to exercise a beneficial influence over the science of medicine. Amongst the names of its promoters we notice those of Dr. Macartney, Dr. Stokes, Dr. Graves, Mr. Crampton, Mr. Colles, Mr. Carlile, Dr. Corrigan, and other well-known cultivators of pathological anatomy.

COLLEGE OF PHYSICIANS.

The London College of Physicians have recently issued a printed paper, in which it is stated that the alterations which have been made within the last few years in its regulations are not properly understood by the public at large, and that their repromulgation is essential. The paper then announces its terms of business. The College requires from candidates for its diploma, testimonials of "unimpeached moral character," of the attainment of the age of 26, and of a 5 years' study of medicine, pursued in a particular order of subjects, including a hospital attendance of 3 years, in Great Britain or Ireland. Candidates from foreign medical schools, must, in addition, have attended a hospital in this country for 12 months. Candidates, already in practice, and aged 40 years, who have not gone through this prescribed course of study, must, before examination, present such other general testimonials as the College may approve. There are to be three separate examinations; the first, in anatomy and physiology; the second, on disease and general science; the third, on treatment; all conducted in Latin, excepting when the Censors' Board selects questions and allows answers to be made in English. The possession of a knowledge of Greek is expected, but not not made indispensable, but translations into Latin from Hippocrates, Galen, or Aretæus, or from Celsus, or Sydenham, &c., into English, will always be demanded. Part of the examinations will be conducted in writing. The College selects for "recognition" no particular medical schools or hospitals. Candidates may have graduated anywhere. Next week we may publish the paper of the College entire, but the document is one of no pressing or particular interest.

CORRESPONDENTS.

The letters of *Mr. W. B. Carpenter, Dr. Marshall Hall, Mr. Slade, Mr. Wright, Mr. Farrier, Mr. Porter, Mr. Somers, Judex*, and *F. R. S.*, have been received. Some of them shall appear next week.

Thanks to *Mr. C.*, of Dublin, for his friendly letter. We have made inquiry, and find that A. R. is not the person whom our correspondent has mentioned.

We have received the letter of *Dr. J. B. Serny.*

In reply to several correspondents, we have to state that medical witnesses at coroners' inquests are not entitled to the fees which are awarded by the Medical Witnesses' Act, unless they be summoned *by the coroner himself*, according *to the form which is prescribed in that statute.* If the *manner* of summoning had not been specified in the Act, medical men would still have remained subject to the officiousness and impertinence of parish beadles. The summonses which are now signed and served by such functionaries may be treated with contempt.

We have received the report of the "Committee of Correspondence" of the Royal College of Surgeons in Ireland.

A Medical Student, in conformity with what is stated elsewhere, must perceive that a "common agreement" would not answer his purpose.

J. J. H. should not answer Dr. Maunsell anonymously.